

## **SOCIOLINGUISTICS**

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C. H. VAN SCHOONEVELD

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# SOCIOLINGUISTICS

Proceedings of the UCLA Sociolinguistics Conference, 1964

*edited by*

WILLIAM BRIGHT

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## PREFACE

As the second in a series of linguistics conferences sponsored by the UCLA Center for Research in Languages and Linguistics, a Conference on Sociolinguistics was held on May 11-13, 1964, on the Los Angeles campus and at the University Conference Center, Lake Arrowhead. Twelve specially invited scholars from other universities met for the occasion with members of the Los Angeles linguistics community and other guests, in what was perhaps the first conference to be exclusively devoted to this field. Proceedings were organized around thirteen prepared papers, most of which were duplicated and distributed to the participants in advance. At each of five sessions, two or three of the invitees presented oral summaries of their papers, and the meeting was then opened to discussion.

The present volume consists of the prepared papers and the tape-recorded discussions, as revised for publication by the participants and by the editor. The Introduction, "The Dimensions of Sociolinguistics", was written some time after the Conference, when arrangements for publication had been made.

For making the Conference and this volume possible, thanks are due to all the conferees; to Prof. Jaan Puhvel, Director of the UCLA Center for Research in Languages and Linguistics; to Jean Grover, Roxana Ma, Suharni Hadikoesoemo and Connie Asbury for secretarial assistance; and to Messrs. Mouton.

W.B.



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## INTRODUCTION: THE DIMENSIONS OF SOCIOLINGUISTICS

WILLIAM BRIGHT

The term 'sociolinguistics' is a fairly new one.<sup>1</sup> Like its elder sisters, 'ethnolinguistics' and 'psycholinguistics', it is not easy to define with precision; indeed, these three terms tend to overlap somewhat in their subject matter, and to a certain extent reflect differences in the interests and approaches of investigators rather than differences in material. It is certainly correct to say that sociolinguistic studies, like those carried out under the name of 'sociology of language', deal with the relationships between language and society. But such a statement is excessively vague. If we attempt to be more exact, we may note that sociolinguistics differs from some earlier interests in language-society relationships in that, following modern views in linguistics proper, it considers language as well as society to be a structure, rather than merely a collection of items. The sociolinguist's task is then to show the systematic covariance of linguistic structure and social structure — and perhaps even to show a causal relationship in one direction or the other.

However, although sociolinguists derive much of their approach from structural linguistics, at the same time they break sharply with one linguistic trend. This is the approach which treated languages as completely uniform, homogeneous or monolithic in their structure; in this view, now coming to be recognized as a pernicious one, differences in speech habits found within a community were swept under the rug as 'free variation'. One of the major tasks of sociolinguistics is to show that such variation or diversity is not in fact 'free', but is correlated with systematic social differences. In this and in still larger ways, linguistic DIVERSITY is precisely the subject matter of sociolinguistics.

To be sure, such a characterization still falls short of suggesting the broad range of sociolinguistic studies which is possible — and which is, indeed, exemplified in this volume. We may perhaps come closer to describing this range by trying to identify the DIMENSIONS of sociolinguistics — the separate lines of interest which run through the field. Wherever two or more of these dimensions intersect, we may expect to find a subject of sociolinguistic study.<sup>2</sup> In the following paragraphs, seven such dimensions are discussed.

<sup>1</sup> An early use is that of Currie 1952. The Third Edition of Webster's *New International Dictionary* (1961) does not list the word.

<sup>2</sup> This approach is modelled after the method of componential analysis current in phonology and semantic analysis, used in specific sociolinguistic contexts by Brown and Gilman (1960) and by Friedrich in the present volume; cf. also Ervin-Tripp 1964.

Viewing diversity as a key concept of the field, it is reasonable that a most important set of dimensions should be related to the *CONDITIONING* of linguistic diversity. This term refers to the various socially defined factors with which linguistic diversity is found to be correlated. The number of such factors may differ from one case to another, but three of them seem to account for most reported cases of diversity: the dimensions of sender, receiver, and setting (cf. Hymes 1962).

1) The social identity of the *SENDER* or speaker is illustrated most clearly by cases of 'class dialects', where speech differences are correlated with social stratification — such differences perhaps reaching their extreme form in the caste dialects of India. The same dimension is relevant in cases of difference between men's and women's speech (Furfey 1944).

2) The social identity of the *RECEIVER* or person spoken to is relevant wherever special vocabularies of respect are used in addressing superiors, as has often been reported from the Orient (e.g. Martin 1964) and from Oceania (Garvin and Riesenbergs 1952). Another special style of speech conditioned by this factor is 'baby talk' as used in English and many other languages — where this term refers, not essentially to the way that babies talk, but to the way that adults talk to babies (Ferguson 1964). Still other types of speech determined by the identity of the receiver are the special styles used by the Nootka in addressing children, dwarfs, hunchbacks, one-eyed people, and uncircumcised males (Sapir 1915). In many cases, a special style used in speaking to a person is also used in speaking *ABOUT* him; but the identity of the person spoken about is rarely, if ever, correlated with an independent dimension of linguistic variation.

3) The third conditioning dimension, that of *SETTING*, comprehends all possibly relevant elements in the context of communication other than the identities of the individuals involved. This is exemplified by the special linguistic usage of Apaches when on the warpath (Opler and Hoijer 1940), or by the differences between formal and informal style which are determined by social setting in most (perhaps all) languages. Where sharp differences in form and function exist between formal and informal style, we speak of a situation of *DIGLOSSIA*; this is found in the Arabic-speaking countries, in modern Greece, Haiti, German-speaking Switzerland, and in most of South India (Ferguson 1959).

It should be understood, of course, that the three dimensions which have been listed are by no means mutually exclusive, but commonly intersect to condition a particular type of sociolinguistic behavior. Thus the so-called male and female speech of the Yana involved considerations of both sender and receiver: 'male speech' was used whenever a man was either the sender or the receiver, while 'female speech' was used only between women (Sapir 1929). The complex linguistic etiquette of Javanese involves the factors of sender, receiver, *AND* setting. It should also be understood that each of these dimensions may have to be broken down into smaller ones in particular cases. For example, usage determined by the identity of the sender or receiver may involve a complex interaction of such factors as age, social rank, and closeness of kin ties, as is illustrated by Friedrich's paper in this volume.



4) Other dimensions of sociolinguistics are based not so much on the actual diversity of linguistic behavior, but rather on the scope and aims of the investigator. Thus, as in other fields, sociolinguistic research can be **SYNCHRONIC** or **DIACHRONIC**. In the realm of the caste dialects of India, we can point to studies of both types: that of Gumperz (1958) focusses primarily on the present-day differences and functions of caste dialect in a Hindi-speaking village; that of Bright and Ramanujan (1964) tries to find historical causes for the differences between caste dialects of South India.

5) A dimension introduced to the discussions of the UCLA Conference by Hoenigswald's paper was that of the difference between how people **USE** languages and what they **BELIEVE** about the linguistic behavior of themselves and others. The latter topic, aptly labelled 'folk-linguistics', is of frequent concern to the sociolinguist. In many parts of the world, for example, the native view tends to confuse 'high vs. low' speech, in the sense of formal vs. informal, with 'high vs. low' as referring to the social status of the sender. In such cases, the investigator must not be deceived into accepting the folk-view as corresponding to actual linguistic behavior; at the same time, he should realize that the folk-view is itself a part of the sociolinguistic situation, and worthy of study in its own right.

6) Another dimension is that of the **EXTENT** of diversity. This term should not be understood as referring to purely geographical measures, nor to simple linguistic measures, such as the number of shared words. Rather it refers to the difference between parts of a single society or nation as opposed to the difference between separate societies or nations, and to the difference between varieties of a single language as against the difference between separate languages. Specifically, three classifications seem to be useful under this heading of extent. One, here labelled **MULTIDIALECTAL**, includes the cases where socially conditioned varieties of a **SINGLE** language are used within a single society or nation; examples are the contrast between 'U' and 'non-U' speech in Great Britain, or between formal and informal usage in the Tamil-speaking society of South India. A second classification, the **MULTILINGUAL** type, includes cases where several **DIFFERENT** languages are used within a single society or nation. This category refers in particular to the problems of multilingual nations, such as Belgium, Ghana, India, Canada, and Paraguay — to take examples from five different continents. The third classification is **MULTISOCIETAL**, including studies of separate languages spoken in separate societies. The aim here is to find correlations between differences in language and differences in social structure, following the lead of the Whorfian hypothesis, which postulates correlations between linguistic structures and the associated non-linguistic cultures. Thus Whorf might have proposed — although he did not — correlations between Hopi grammar and Hopi social organization, to be contrasted with correlations between English or SAE grammar and SAE social organization. An actual study of this kind is Fischer's paper in this volume, in which the Trukese and Ponapean languages and societies are contrasted.

7) A final dimension to be recognized here is that of APPLICATION — the broader implications which are inherent in descriptions of sociolinguistic diversity. Again, three categories may be recognized, corresponding to the interests of three types of investigator.

The first application, reflecting the interest of the sociologist, involves the use of sociolinguistic data as a diagnostic index of SOCIAL STRUCTURE in general, or of particular social phenomena. Thus, the recognition of a three-way division of caste dialects in South India, plus a two-way distinction of formality, may be correlated with other kinds of data to yield a description of the socially defined differences between people and the socially defined differences of setting which are significant for South Indians. Furthermore, once the socially relevant classifications of people and situations have been recognized in this way, the investigator can use linguistic criteria in order to classify particular individuals and situations: a man who speaks in such-and-such a way reveals himself as a Brahmin; an occasion on which such-and-such language is used is recognized as a formal occasion.

The second type of application reflects the interest of the HISTORICAL linguist. The questions posed here are: Do languages change in different ways under different social circumstances? Do different social dialects of the same language change at different rates or in different ways? How does the history of a language reflect the interaction of social dialects? The study of these questions can be undertaken either by the examination of historical records, where available, or — better still — by studying the currently ongoing processes of linguistic change, as Labov has done on Martha's Vineyard and in New York (1963, 1964).

The third type of application is that made by the LANGUAGE PLANNER — the linguist, educator, legislator or administrator who must work with official policies regarding language use. Thus, given an organized society in which a diversity of dialects or languages are current, the language planner must consider such questions as: What varieties are to be given recognition as 'official' or 'national' languages? What varieties are to be sanctioned for use in official publications, in officially encouraged literary work, in educational institutions of various levels, in courts of law? What should the official attitudes be toward varieties not sanctioned for any of these situations? To what extent should political subdivisions of a nation correspond to linguistic subdivisions? How should writing systems be developed or standardized? The most complex problems of language planning, perhaps, are those faced by the recently independent nations of Africa and Asia, and the diverse policies being followed by these nations provide current illustrations of the types of difficulties which arise.

The papers and the discussions presented in this volume reflect the above dimensions, intersecting in a variety of combinations. Clearly, all the possible combinations have not been exhausted; many new lines of research remain to be developed, along with the collection of new data on problems already raised. But the prospect is very bright for all of the possibilities to be explored. Sociolinguistics has recently been the topic not only of the UCLA Conference, but also of a symposium in San Francisco in 1963

published under the editorship of Gumperz and Hymes (1964), and of an eight-week seminar at Indiana University in 1964, from which a variety of publications are expected. A reader in the field, edited by Joshua Fishman, is announced for publication. It seems likely that sociolinguistics is entering an era of rapid development; we may expect that linguistics, sociology, and anthropology will all show the effects.

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## A PROPOSAL FOR THE STUDY OF FOLK-LINGUISTICS

HENRY M. HOENIGSWALD

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It would be idle to spend much time restating and extolling the closeness of the ties that link linguistics with its sister social sciences, in particular with anthropology. The existence of these ties is a truism. But it is quite a different proposition to have a look at the way in which the truism itself is generally presented and regarded. If you will, this is a problem in the (recent) history of scholarship. What conceptions do linguists and anthropologists harbor of each other? What particular aspects of one's labors catches the fancy of one's colleagues across the partition? What has been the special role of individual workers trained in both fields? How are contacts made? How do concepts travel across, and what happens to them as they do? What part do misunderstandings play? There are many reasons why I could not begin to attack these questions on a broad basis. Some reasons are general: it is always bad to tackle intellectual history in terms of contemporary preoccupations, working backward from them. The danger is myopia, with superficial issues looming large and concealing the really important lines. Some other reasons are special, even personal ones — including, in the present case, a painful degree of anthropological ignorance. I can only hope that this ignorance does not mislead me when I believe that the few items which I propose to mention here have not received adequate attention and therefore deserve something like an opening statement. At least it is true that this must seem so to a mere linguist, and perhaps there is sufficient excuse here for a plea. In any event, rather than stay for any great length with these generalities, I intend to turn to a number of very mundane practical points which appear to me to have been especially neglected.

Before leaving the generalities, however, there is the exasperating bit of folklore according to which linguistics is 'ahead' — in precision, elaborateness (or simplicity?) of concepts, concreteness of results — and even, according to some, in the degree of unanimity with which the practitioners allegedly accept each others' descriptions, analyses, and inferences. Of course, this kind of admiration from afar is not universal; when it is offered it should at the very least be tempered with the observation that the presumable gain in formal power may come at the expense of depth of subject matter. Language is only language and (another fond motif) as such only a 'part' of culture. The transferability of formal linguistic notions to the 'other parts' of culture is certainly not a simple proposition; the most successful examples are easily those in which the

analogies are somewhat loose. Language may be a part of culture, but not necessarily a typical one; and the fact that it is precisely the study of language which has tended for so long, and on the whole so fruitfully, toward some kind of formalism, is certainly no accident. Rather it must signify something deeper than mere greater 'simplicity' taken as a matter of degree.

There is a habit, in itself honorable, indispensable, and deeply ingrained, of discounting informants' pronouncements on their language. In extra-linguistic matters it is enough to understand that there must be a distinction between the ideal and the real culture, whereupon the investigator is free to study each separately or even jointly. In the case of language a past unhappy history has prevented that; the necessity to warn against confusing the facts of speech with the talk about speech has been too real to allow much serious interest in the latter. Thus it has come to pass that (I am told) there exists a sizeable body of information on popular belief concerned with plant life, weather, health, even social institutions and history; there is ethno-zoology and ethno-medicine, and all these things have been subjected to study in their own right; but belief about language is different, although data are not wanting. There are many cultures in which articulateness on this point is overwhelming and far outstrips many other possible areas of interest. It is of course precisely this articulateness which has caused all the trouble. What follows is not designed to provide a framework for dealing with this area, but only to formulate a few lines of inquiry with which, perhaps, to supplement standard field work instructions.

There is actually no point, at any stage, in avoiding a confrontation of elicited cant with the real and therefore concealed facts. For this reason we shall assume, ideally, that the investigator has either made, or has left room for, a study of the latter facts. This would include a synchronic grammar and a dictionary. It would further include a statement of the typological and genealogical (that is, historical) position of the language. It would further include a description of the structure of the speech community, not only in general anthropological terms, involving the structure of the group that happens to speak the language, but particularly with regard to speech differences, be they social or geographic.<sup>1</sup> Furthermore there will have to be information on linguistic contacts with outside groups, matters of intelligibility, multilingualism, etc. It will also be of particular value to know as much as can be known about quasi-linguistic behavior manifested in gestures, overlaid voice qualities, and the like.

The next step might well be a report on the nature of speech acts. Here it is notoriously hard to decide what is still part of the grammar (or the style book) and what is part of the setting. For instance, we might ask how dialogue proceeds, to what degree there is predictable use of formulae or predictable response. What is the function of oratory and of soliloquy? What other formalized uses of language do exist: story telling, poetry, punning, ritualistic use of words? Is there a premium on literally

<sup>1</sup> More or less in the atlas tradition as developed by McDavid, Labov, etc. Perhaps the time will come when community studies will no longer exclude linguistic data as a matter of course.

(i.e. phonemically) exact reproduction of language matter, with memorization being a central skill? Is there writing and for what purpose is it practiced?

In addition there should by all means be a semantic 'field' study concerning the vocabulary referring to speech activity. It is obviously difficult to give specifications without prejudging the case. Since, however, such investigations must largely be done on the basis of a given semantic typology and without benefit of explicit controls, it will be well to suggest a few possible dimensions. The investigator might do worse, for instance, than to start out by collecting expressions for the speech act, and studying their synonymics. In doing so he will come across pairings that might be suggestive. One part of the world in which this is true would be that in which speech and thought, words and ideas are contrasted or linked. Another feature which would certainly stand out in our own tradition would be that of treating speech as a superficial expression of something more fundamental behind it. Ours is the culture in which mere words are looked down upon and unfavorably compared with things.

In certain areas it will be rewarding to wax more technical, if there is anything approaching a formalized activity in which language is the object of reverence, contemplation, or study. Here again the first approach is necessarily by way of vocabulary. In terms of translation from English, one might begin by asking for the equivalents of items like 'word' or 'sentence', probably the only two grammatical terms which at the same time have real currency in western culture. If the experience from western culture is worth anything, it might be rewarding to investigate such equivalents for their actual fit. To what extent are the terms in question objectively justifiable? Is it even a fact that they are used in anything like a definable sense? If the area under investigation were the English speaking world, the answer in the case of 'sentence' would be much more positive than in the case of 'word'. We are reminded of the old observation to the effect that the Greeks had no word for 'word' (although the Romans did). We also feel impelled to look for impurities in the conditioning of subjects who, to name but two possible aspects, may be discoursing about spelling and punctuating if they are literate, or about scanning and chanting if they have an institutionalized poetic craft to draw on.

We may further find it easy to prompt reactions to and discussions of synonyms and homonyms. Our main concern should again be to match the informant's own description with the linguistic analysis of the data.

Questioning ought then to proceed in the direction of discovering attitudes toward speech differences, starting perhaps, for obvious reasons, with gross differences. There is probably no difficulty in recording what the subject has to say about peculiarities of other languages and dialects. On the other hand it is presumably too much to expect anything coherent from a correlation of such clichés with the true typology of the area. But it would be good to find out something about the subject's ability to distinguish correctly between several languages not spoken by him, and about the shibboleths in use. All these questions may be expected to take on a different coloring when it comes to forms of speech understood by the subject, either because he is

bilingual or multilingual or because the speech forms in question are only dialectally different from his. Here there does appear to be a good deal of interest in knowing how well the subject's judgements and opinions tally with the truth. One might even go so far as to examine informants for their capacity to imitate, in jest or earnest, foreign habits with any degree of success.

This leads to a further point. We should discover from bilingual subjects how they themselves look upon bilingualism, both statically and, if this is applicable, in autobiographic retrospect with regard to the way in which the second language was acquired. There must be wide variations here, ranging in degree of awareness from infancy-acquired bilingualism to institutionalized study. A related question is posed by interpreters' activities. Under what conditions are expert translators called upon? What kind of people are they? What are their practices and traditions?

Subtler than this are attitudes and reactions to the internal structure of the speech community. It may be that our informants believe in speech differences by age; they may also, for instance, be convinced that women speak differently from men, and on those points they may either be right or wrong. The investigation of the social role of speech differences is, to be sure, a legitimate part of the objective study of what GOES ON rather than of what is SAID about it, although scraps of the latter kind always intrude. Perhaps it is more to the point to observe, in addition, how speakers BEHAVE in the face of a dialect difference. Their reaction may be explicit or implicit (with either a correct explanation or an incorrect rationalization in the first eventuality). If it is implicit, it is almost sure to be beyond the grasp of hurried questionnaire work. As for one special, probably widespread type of explicit reaction: when and in what terms is CORRECTION performed? How, in particular, is the speech of infants and children reduced to conformity? This may open up a further level of derivative phenomena: if formulated ideals of correctness and acceptability exist, to what extent do corrective activities (taking notice, ridicule, insistence on repetition, censure) conform to such ideals? And here is a further line of questioning: while it may be difficult to determine the weight given to speech characteristics when it comes to the acceptance of individuals in the group, it is relatively easy to observe what ideals are abstractly cherished on this point. Are taciturnity, facility, or inventiveness prized properties? Does a person earn praise for being 'well-spoken'? And so on.

As I remarked before, in order for the speaker to achieve awareness it may be necessary to encounter and manipulate linguistic material in a setting which is part of some other facet of the culture. Words and phrases may be isolated and handled on a conscious level because they function in poetry, song, punning etc. An aspect that requires particular attention is of course mythology and religion. As the investigator turns to those areas he may expect data having to do with word magic and with taboo. He is not likely to miss the importance and, if he can penetrate it, the nature of a living system of vocabulary taboo. But he should beware of wrong interpretation: if a euphemism has succeeded in replacing the tabooed item altogether, the process has become a matter for the historian rather than for the synchronic analyst.

It is convenient to add here a number of possibilities having to do with the esoteric interpretation of language phenomena, some of them pathological or marginal. Here the folklore indices might provide expert guidance. First of all there may be a body of beliefs on the relation between names and things, or between creation and naming — connected or not, as the case may be, with the idea of a prehistoric race speaking a special language, or creating language itself. There may also be beliefs concerned with a secret language characteristic of demons or spirits, with animal language, and with special powers to understand such special languages vouchsafed to certain individuals at certain times. And finally there may be an interest in true abnormalities. What folklore is there about stammering and stuttering, about muteness, and about speech deficiency appearing either chronically or suddenly as a symptom of illness?

One would hesitate to put forward suggestions of this kind if there were an established and consistent practice. If other phases of social science are any indication, we should be interested not only in (*a*) what goes on (language), but also in (*b*) how people react to what goes on (they are persuaded, they are put off, etc.) and in (*c*) what people say goes on (talk concerning language). It will not do to dismiss these secondary and tertiary modes of conduct merely as sources of error.

#### DISCUSSION

**HYMES:** This is a very important subject that Dr. Hoenigswald has presented, partly, I think, because if we're going to promote something called sociolinguistics, it should be moulded in a broadly conceived frame of reference rather than a narrow one. And Dr. Hoenigswald has brought in some aspects of the place of language in society which might be otherwise neglected. He's opening up here the whole question of the values of a culture, and the beliefs and knowledge of a culture, with regard to language and the way that those affect the place of language in society. Dr. Hoenigswald's approach in this paper has a fresh and rather different point of view than the one in my paper on the ethnography of speaking (1962). In that paper I approached it as simply a problem in ethnographic description. I tried to outline some of the considerations which would be important if you were to do an ethnography from the point of view of the role of language in society, the same way that people do ethnographies from the standpoint of religion or economy or social structure. Here Professor Hoenigswald is trying to get to the history of linguistics in a special way, as he has pointed out, and also to get to what have been called secondary and tertiary reactions to language. I think this is a very good way of introducing these questions from the point of view of communicating an interest in them to linguists in general.

**HAUGEN:** One interesting aspect of this paper is its use of the term 'folk linguistics'. Since there is an 'ethnobotany' and an 'ethnomedicine', this ought perhaps to be called 'ethnolinguistics', since the function of that term has actually been taken over by 'sociolinguistics' these days.



The terms that Professor Hoenigswald makes use of at the end of his paper, 'secondary' and 'tertiary' reactions, stem from a famous article by Leonard Bloomfield (1944). Bloomfield's attitude to these reactions was definitely negative. In fact, people were going around using 'tertiary reaction' as a pejorative term because of his article. I have always found sociolinguistics an extremely interesting field and I am happy to find it gradually becoming respectable. Many years ago I read a book called *Wie denkt das Volk über die Sprache?* (Polle, 1889), a well-known classic of European linguistics, though I don't think it has ever been mentioned in this connection. It led me to think about the subject and to include questions on it in the questionnaires that I used in my field survey of Norwegian dialects in America. I asked people to tell me what they thought about their own dialect, about other dialects, and about the standard language. I discussed some of the results in my book on the Norwegian language in America (1953). I found that people who speak folk dialects are not unanimous in their attitudes towards these dialects. Some dialects are regarded as beautiful and desirable, others as undesirable and ugly, even the ones that the person himself speaks. An interesting job would be to find out by what social channels these attitudes are transmitted.

Then there is the field of folk etymology, an established branch of linguistics. I wonder whether folk etymology should have any place in your 'folk linguistics'. Certainly, it is something that has actual linguistic results.

FERGUSON: I was pleased to hear Dr. Hoenigswald call our attention to the importance of the study of the attitudes people have toward language. I just wanted to comment that work is often done in this field not by people who are concerned with pure research interests, but by people who are in applied linguistics jobs. For one example, there are people who are concerned with literacy programs or educational development in developing countries, who have to cope with the differing attitudes of different societies on matters of literacy and language usage. In two Arab countries there are substantial minorities of non-Arabic speaking people, Kurdish-speaking in Iraq and Berber-speaking in Morocco, and the attitudes that these communities have toward language are very different. Many Kurds insist on developing Kurdish as a written language and a medium of education, whereas in Morocco very few Berbers would insist on having Berber used as a written language; they prefer Arabic, which is somehow 'better'. They want to have schooling in Arabic, not Berber. People who are planning programs of that kind are forced to give explicit recognition to attitudes and beliefs.

Another example is familiar to almost all of us here — the teaching of exotic languages in this country, where a linguist deals with a native speaker of the language and has to cope with the native speaker's attitudes towards the language, with his own American students' attitude toward the language, and with his own linguist's attitude toward language. Often these attitudes are not very much in line, and I for one have been forced to make a series of explicit statements about what Arabs believe about their language. One could easily make a similar list of statements about the attitudes

of one's students toward the language being learned or to language learning in general. People in the applied business very often are forced to take account of language attitudes.

HOENIGSWALD: Just for that reason, don't you think that a manual for eliciting would be a good thing?

FERGUSON: Yes, I do, and there have been some tentative starts toward manuals for this kind. A very crude one, which is a second remove away from this, is the series of questions that are asked by the person who is writing a language chapter in an HRAF book. This manual, prepared at American University, asks a number of very clear questions about what attitudes there are toward language, what kinds of writing there are, what people think about this, etc. But this is essentially for use in asking the expert, who presumably has all this knowledge, rather than eliciting directly from the people.

FRIEDRICH: I'd just like to add one substantive comment to the remarks of Dr. Hoenigswald about the development of folk linguistics. I recently ran across some very interesting material about what I would call folk-linguistics in a book by a man called Zahan, called *La dialectique du verbe chez les Bambara* (1963). These Bambara, Africans, have first of all worked out the basic linguistic structure. They have inferred the vowel phonemes and the consonant phonemes, etc., but they've also related them to other things. Just the vowel phonemes will show this. You have the familiar *i*, *e*, *a*, *o*, and *u*, but these are related to the numerical system 1, 2, 3, 4, 5. Then these are related to the key concepts in a folk psychology which is also very well developed; name, tenacity, celebrity, moral force, and solidity respectively correspond to the five vowels. These in turn are related to the five fingers — the little finger, the ring finger, middle finger, index, and thumb. This in turn is hooked up to the folk biology, the folk anatomy, and finally this whole set of correlations is linked to the analysis of the verbal structure. So you not only have here a folk linguistics in the sense of a complete system, but it's a typical system such as one finds among primitive people where you don't have compartmentalization. The linguistic structure is hooked up to many other types of structures.

SAMARIN: In connection with what Dr. Friedrich just said, I might mention that Mme. Geneviève Calame-Griaule is apparently doing a similar study on the Dogon of the Republic of Mali. If I have understood correctly, she is writing a doctoral dissertation on the quasi-philosophical aspects of the Dogon attitudes to their own language.

In connection with Dr. Hoenigswald's paper, I wonder how much folk linguistics, if we can call it that, there is in theology, and perhaps even in philosophy — at least in the kind which is known as 'linguistic philosophy'. These disciplines are extremely sophisticated, I know; but I can't help feeling, on the basis of my own reading in theology, that some common-sense notions may persist in the way some of these people use language data. In any case I think that it would be very interesting to investigate the linguistics of religious systems. Buddhistic, Islamic and Christian literature provide us with plenty of data.

In this connection I have also been interested in the words which are available in different languages for talking about these languages. This study is really part of my interest in a field linguist's metalanguage. It seems to me that there might be some correlation between what people say about their language and the words they have to use. Preliterate societies seem to have a single word to cover everything from the name of an object to a whole speech. The Gbeya do. This takes me back to folk-linguistics and theology because there still is a lot of dependence on the 'word'. Theologians give the impression that everybody knows what the word is.

HYMES: There is a very interesting book by Kenneth Burke, who is a literary critic, called *The Rhetoric of Religion* (1961). In it he takes the point of view that theology is a verbal discipline *par excellence*, and that this is in no way a criticism of it; it's simply saying that the material with which it works must be linguistic. He presents a number of theses, and analyses of particular writings (St. Augustine's *Confessions*, the first three chapters of *Genesis*) in terms of the use of language and assumptions about language. Another good source is Kenneth Pike's book, *Language and Life* (1958), in which he works out a parallel between trinitarian theology and linguistic structure.

Regarding what must seem to us the linguistic 'misconceptions' of Protestant theologians, I think that theology, and, one might add, ritual, the rules of monastic orders, and the rest of religion, constitute an interesting and possibly revealing area of study. We should be very careful in this area, however, especially if we call it 'folk-linguistics', not to assume that we already have the truth as a standard of comparison for other conceptions of the nature of language. This is true for sociolinguistics in general. As Dr. Hoenigswald, I am sure, could tell us, there are many examples in which, if you found out what many of the great linguists of the past thought they were doing, as opposed to what we now see them as having done, you would initially put their beliefs in the category of amusing misconceptions. Many of our own pet notions will probably end up in that category in the view of future generations. I think that perhaps some of those who seem to us so off-base in their approach to the use of language may be nearer the mark than some of our contemporaries and recent predecessors.

LABOV: I particularly appreciate the broad view that comes from Dr. Hoenigswald's paper. I've utilized some of the themes he mentions in several communities where I've worked. We might well distinguish between two types of subjective attitudes, those which are overt and those which are unconscious. Now the unconscious responses that I have worked with are extremely consistent and uniform; in a sense they are quite parallel to the emic structure of the language with which we are familiar as linguists. The overt responses in American and English society generally are quite poor as far as vocabulary is concerned. 'Poverty-stricken' would be the best term for this vocabulary. The inadequacy of people's overt remarks about their own language is directly reflected in the fact that there are only a few words that they use to convey the subjective response that they feel. For example, 'nasal'. Frequently, if you ask

somebody what he thinks of this style of speech (nasalized), he'll say it's very 'nasal'; and if you produce a speech of this sort (denasalized), he'll say that's very 'nasal', too. In other words, the denasalized speech characteristic of some urban areas and extremely nasalized speech are treated in the same way. The word 'twang' is another broad spectrum word. Anything which is distinctive about a dialect may be referred to as a 'twang'. Sometimes this is associated with nasality, and sometimes it isn't. In Martha's Vineyard there was frequent reference to something called a 'Chilmark twang', and folk linguistics could not penetrate this expression. It was simply a phrase that was used to identify a global subjective response without any further vocabulary to analyze it. But some of the references made here today show that there are highly institutionalized folk attitudes toward language which are much richer than those which we are accustomed to meeting in the U.S. and in England. I think that institutionalized attitudes are an exceptionally rich area which shouldn't be neglected. When we elicit subjective responses which people have never considered before, we are doing something quite different from investigating practiced responses which are there whether we request it or not. In eliciting new, ad hoc responses, I think that we can mine a very rich vein of information.

Grootaers (1959) did a fine study of the attitude of Japanese villagers towards the dialects of surrounding villages. He had approached the villagers in a mountainous area in Japan and said, "Going north, where do you find speech which is different from your own, and how different is it?" He worked out a quantitative scale going north, east, south, and west, and superimposed the reactions of large numbers of villagers upon each other to produce an overall map. Then he became quite discouraged with the method. He said the patterns were of little value because they did not coincide with the real speech differences. With all the ingenuity of his approach, he had somewhat naïvely expected that the folk-attitudes would coincide with the objective differences, and so he discarded the method which I think might have led to very significant results. Perhaps that matches the problem that Professor Haugen mentioned — that rejection of secondary and tertiary responses was partly due to confusion between the use of these attitudes as objective information in itself, valuable for the study of the development of language, and the rather futile attempt to utilize these attitudes to describe speech production itself.

GARVIN: I want to stress the importance of the distinction between attitudes towards language and actual linguistic patterning, particularly in view of the recent emphasis on explicating native speakers' intuition. I think it is very important to keep two things in mind. One thing is how much of the intuition of the native speaker reflects his folk attitudes towards language and how much of it reflects actual linguistic patterns. The second is, when the linguist is a native speaker rather than an analyst, to what extent does he then share folk attitudes and pretend that they are linguistics?

HOENIGSWALD: That's exactly why I got interested in this. I wanted to know how many unformulated preconceptions go into linguistic theory.

PAPER: I also was very much interested and stimulated by Dr. Hoenigswald's call for a kind of formalization of eliciting these attitudes, because anybody who has worked with any kind of language, whether as an analyst or even in a normal exposure to other linguistic systems, for instance as a traveller, immediately picks up a great amount of folklore about attitudes towards language. In fact, it is hard for me to imagine anyone, in a normal language teaching or learning situation, not having to be told explicitly from Lesson 1 something about the socially conditioned variation in speech. My own experience in teaching Persian, for example, has forced me from the very first day to explain the differences between the parameters of formality vs. informality, and of written language vs. spoken language, and the various kinds of speed styles. Of course most naïve speakers of English don't know what you are talking about. You'd better start off explaining their own practices in English. In my own experience many Western European speakers of French or English who have spent many years in Iran have consistently made the mistake of identifying, in Persian, the distinction between formal and informal with the Standard Average European distinction of standard and substandard; and this doesn't work at all. They are quite incommensurable. I suppose everybody in this room who has dealt with even a few languages comes across these things and has amassed this information in a kind of 'off the cuff' collection of linguistic folklore. In the case of Persians' attitude toward speech it is well known that they value poetry very highly; if you ask a Persian speaker to give you the definition of a word he usually will try to quote a couplet of poetry in which the word occurs. Also, if you ask English speakers to identify European languages, for example, I suppose the response would be almost 100% that German is guttural, Italian is beautiful, and French is musical or something of this sort. We all know these; we could almost rank languages in terms of difficulty unerringly, usually with Chinese coming at the top and Spanish coming at the bottom, where the top is most difficult. I agree with Dr. Hoenigswald that we certainly ought to try to codify these in some way, if only at first in a kind of data-collection sense.

BRIGHT: This confusion between formal and informal on one hand, and standard vs. sub-standard on the other hand, is also prevalent in India. Not only the run of European visitors to India, but even some linguists have been taken in by the Indian attitude that colloquial speech is in fact somehow equatable with European substandard speech. It's something that a linguist has to watch out for, not to let himself be misled by the attitudes he will hear from his Indian informants.

FRIEDRICH: I'd like to make explicit something that I think has been implicit in a lot of the comments so far. In anthropology today there's a very considerable concern with the dichotomy between the ethnographer's or the anthropologist's view of reality — the analytical system that he abstracts — as against what is called the native model, the picture that the people themselves have of their cultural system. One point in the discussion so far is that native models of language seem to be peculiarly deficient. I think this ties in very nicely with the well-established fact that language, of the great institutions of man, is the most implicit in the subconscious, the most automatic.

HAUGEN: Several of our speakers have referred to the 'folklore of language', and this brings up the use of the term 'folklore' as well as 'folk linguistics'. In our culture 'folklore' contrasts with 'science' as being untrue: 'science' is true, 'folklore' is untrue. This is part of our scientific folklore. This is where Dr. Hoenigswald started, namely in discovering that as you go back into the history of linguistic science, it becomes more and more folklorish, and the problem is how to distinguish between two things: On one hand, we are dealing with the covert attitudes of people, which influence their language choice or their language use. On the other hand, there is a set of beliefs that have been consciously inculcated by some source or other, things they've read or heard and which have become part of their folklore, and are overt in the sense that they can talk about them.

HYMES: With regard to the matter of terminology — in those areas which Dr. Hoenigswald has referred to, such as botany and zoology, where we don't have any doubts about the scientific status of the second part of the term, you find both 'folk-botany' and 'ethno-botany', etc., being used almost interchangeably as names for the discipline. People don't worry about a subject's being dismissed as mere folklore just because it's prefixed with 'folk'. Also, as Hoenigswald pointed out, the study of terminology in these disciplines, this whole interest in what is called 'folk-taxonomy' or 'ethno-science' (using either prefix again), is like linguistics in the sense that you use the informant's responses as behavioral attestations of an underlying system which you don't necessarily expect the informants to be able to verbalize. It should be possible to cut across this distinction between conscious and unconscious attitudes, and simply take the whole attestation of behavior with regard to language use as the subject matter for our type of description.

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## ON THE ETHNOLOGY OF LINGUISTIC CHANGE

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Linguistic change, as the term is commonly used, may refer to two kinds of endeavor: internal history, or historical linguistics — the reconstruction of proto-languages from modern historical texts — and external history or 'language history' (Malkiel 1953), which considers changes in linguistic form as they relate to the socio-economic environment. Both types of study have received considerable attention in the past, but it is the former which has benefited most directly from the application of linguistic methodology and which has shown the most significant results. A century and a half of research in this field has led to the development of techniques for establishing sound laws, or sound correspondences, which have proved to be remarkably reliable indices of prehistoric relationships. Although many questions of applicability to particular cases remain, the general principles of comparative reconstruction are well known; results are easily reproducible, and the method is accepted as an important tool of linguistic and historical research.

The subject of external language history, on the other hand, has, until recently, remained outside the scope of formal study. Considerable literature exists on the effect of political and economic change on verbal behavior, on the rise of standard languages, language shift, the replacement of one language by another, the formation of 'mixed languages', pidgins, creoles, trade jargons and the like. The evidence presented leaves no doubt that these developments are attributable to social causes and not to human anatomy, climate, or geography, as had previously been suggested. In a number of cases, correlations with social change have been pointed out. But we still lack a theory of language and society which would explain how specific factors in the social system may lead to linguistic changes, and how linguistic structures are affected by these factors. Furthermore, until recently structural linguists paid little attention to geographically or socially distributed variants within a single culture area, a primary source of data for the linguistic study of language history. Their concern with methodology led them to concentrate on single homogeneous grammatical systems. Students of verbal behavior in specific communities, on the other hand, tended to deny the applicability of formal methods to their field of enquiry. Language history has thus remained marginal to both disciplines, a topic to be discussed, as Malkiel phrases it, "in a friendly conference room where linguists readily discuss current problems of broad implication with historically minded fellow scholars" (1953) — but hardly a subject for formal investigation.

There are signs that these attitudes are changing. Social scientists have become increasingly aware of the value of linguistic indices in their studies. Interest is reviving in language as an index of social stratification and in the role of language change in the process of modernization. A recent major research effort brings together several extensive case histories of language shift in American minority communities. Linguists, moreover, are showing a growing concern with relating grammatical structure to the broader aspects of communication. As we learn more about the details of verbal behavior in linguistically diverse communities, it becomes increasingly clear that the bounds of particular languages or dialects do not necessarily coincide with those of socially defined communities (Hymes 1962, Gumperz 1962), and that speech variation and language contact are often best treated as intra- rather than inter-group problems. A single speech community may contain within its bounds relic groups, preserving forms that have long gone out of use elsewhere, and innovating groups undergoing rapid change. Furthermore, speakers in bilingual societies show varying degrees of competence. Some control both languages equally well; others regularly speak one and have only a smattering of the other. Comparative analysis of the speech behavior of all such populations in relation to their social characteristics should reflect ongoing processes of linguistic change similar to those heretofore studied primarily through textual analysis.

The present paper reports on one such study. It attempts to describe intra-community processes of change through use of methods of interviewing, participant observation and controlled experimentation, such as are ordinarily employed in anthropological fieldwork. We take as our point of departure the study of linguistic forms, but we evaluate the distribution of these forms in social terms.

Any such effort, of course, presupposes some underlying theory of the processes which govern the diffusion of linguistic forms. In fact, most previous studies also rely on such a theory, either explicitly or by implication. By far the majority of studies of linguistic change, however, operate with a simple hierarchical model of society, in which populations are seen as segmented into a series of discrete groups differentiated by means of such categories as class, caste, occupation, sex, etc. Within this structure, innovations are said to spread down the social hierarchy from one dialect or language to another through prestige imitation, a term used to denote a situation in which the lower, or least prestigious groups adopt forms used by their superiors. The underlying assumption is that to each category there is a specific form of speech, or idiom, distinct from all the others.

We find evidence in support of this view in historical texts, where forms which at first remain confined to the upper classes are later generalized and become part of ordinary colloquial usage. But this model does not account for behavior in actual communities. If we apply the theory to a speech community, defined following Bloomfield (1936) in terms of frequency of interaction among individuals or subgroups, then unidirectional diffusion of innovations from top to bottom should lead toward linguistic homogeneity over time.



Field experience, however, tells us that speech communities are always diverse. Although regions may vary with respect to the total extent of internal language distance, some degree of stylistic or dialectal variation always exists. Accordingly, a somewhat modified hypothesis was proposed by Fischer (1958), who suggests a two-step process of change: low prestige groups imitate their social superiors in order to become more like them, but the latter, in turn, introduce new changes to maintain their separateness. A speech community, in these terms, is a system in equilibrium, in which speech differences reflect the 'protracted pursuit of an elite by an envious mass, and the consequent flight of the elite'.

This kind of theory allows for both the elimination of old distinctions and the creation of new ones, and could thus account for many cases of language shift, and for the spread of standard languages. It has often been observed that the very processes which result in the replacement of a dialect or minority language also lead to the formation of new occupational and social elites and to new linguistic distinctions based upon these. But a number of questions remains unsolved. We have no explanation for the many cases where prestige imitation does not seem to operate. Emeneau (1962) cites, in particular, the case of the Toda, the Kota, and the Badaga of South India. These tribes have lived together in a caste-like relationship for several hundred years. The Toda are clearly at the top of this social hierarchy, with the Kota and Badaga performing specific services for them. Nevertheless, each group continues to speak its own distinct language. Similarly multilingual groups can be found throughout Asia and Africa, as well as in North and Central America. In all these groups, if the combination of prestige imitation and change in elite speech were to operate, we would expect lower groups to give up their language(s) and to adopt a deviant dialect of the prestige language. Yet this is not the case.

Other questions arise in regard to the direction of diffusion. In their study of caste dialects of Tamil and Tulu in South India, Bright and Ramanujan (1964) found that innovations in lower-caste dialects cannot be explained on the basis of borrowings from Brahman dialects. They therefore suggest that in a caste society different groups may innovate independently.

These objections suggest the need for a more sophisticated approach to the question of diffusion. Here linguistics shows its debt to social science, for it is the social components of the model which stand most in need of revision. First, the implications of such general terms as prestige for actual verbal interaction must be spelled out. Second, the relationship of the social categories on which the hierarchical model of society is often based to actual groups in societies is far from clear. Third, techniques of linguistic interviewing must be adapted to the revised model.

As a first step toward refinement of the theory, we approach the problem not in terms of historical process, but as a problem in verbal behavior. It has been noted that from the point of view of social interaction, the distribution of linguistic variants in actual communities generally takes two forms (Gumperz, 1962). DIALECTAL or inter-personal variation occurs among socially or geographically distinct groups. SUPER-

POSED or intra-personal variation, on the other hand, reflects shifts in the language of single individuals. Dialectal speech features reflect an individual's personal history. They indicate his ultimate family origin as well as any subsequent changes in group affiliation, i.e., whether he has moved from one area to another or whether his social position has changed in some way. Superposed variations mirror the activities in which an individual regularly engages in his daily routine. Whenever he speaks, he selects from a range of alternative modes of expressing his intent, using, for example, one set of forms with his employer, another with his colleagues, and perhaps yet another with his children. In a monolingual society, selection is limited to alternates of the same language. In bilingual or bi-dialectal societies, however, choice between two dialects or two languages may fulfill social functions similar to stylistic alternation in monolingual societies. Ultimately, choice of linguistic form is always a matter of individual freedom, but to the extent that they carry commonly understood connotations, such choices must conform to institutionalized ways of symbolizing social relationships (Gumperz 1964). We therefore need advance information about the totality of dialect, languages, and speech styles employed in the community, and about the rules of linguistic etiquette which govern their use. Furthermore, patterns of superposed variation vary from sub-group to sub-group, even within the same community. Hence, populations must be carefully defined.

A study designed to elicit both superposed and dialectal variation may be planned in two ways. It may take the form of a large-scale survey based on random sampling in accordance with social criteria established through coördinate sociological research (Labov 1964). Or it may take the form of an anthropological study of one or more small communities, as in the present case.

The community in which these methods were employed is Hemnesberget, a commercial settlement of about 1,300 inhabitants in the middle of the Rana Fjord of Northern Norway. Until the 19th century, the Rana area, located in one of the most sparsely settled regions in Europe, was largely controlled by a small elite of landowners, merchants and officials. These owned vast tracts of land and exercised an officially sanctioned trade monopoly in the area. Great differences in wealth and education separated them from the majority of the population, who were tenant farmers, fishermen, estate laborers, and servants. In 1868, trade monopolies were abolished and land gradually turned over to settlers. The region is now one of small farmers, who earn their livelihood through dairying, lumbering, boatbuilding and fishing.

During the last decade there has been an increasing tendency towards specialization and accumulation of craftsmen and workers in the two local commercial centers of Mo and Hemnesberget. Thus as a result of the abolishment of the trade monopolies and the increasing profitability of the boatbuilding industry, which exports to the forest-poor fishing districts further north, Hemnesberget grew from 100 to 1,000 inhabitants between 1865 and 1900. Several ecological factors contributed to this development. Before the post-war period of road building, Hemnesberget was the natural communication center of the area. It was located in the middle of the fjord,

and had an ice-free harbor providing ready access to the open sea during the winter. It has been a religious center ever since the middle ages. Furthermore, fishermen-farmers used to store their boats and gear for winter fishing there.

Government-sponsored economic development during the last three decades has turned the Rana region into an important iron and steel producing center. The area of Mo-i-Rana at the head of the fjord has grown from about 1,000 inhabitants in the 1920's to almost 20,000 in 1960, largely through immigration from Southern Norway and the region of Trondheim. The city of Mo reflects this growth through the several department stores, hotels, restaurants and cinemas. A railroad from Trondheim to Mo-i-Rana and on to Bodø was recently completed, and the road system is steadily improving. But Hemnesberget remains relatively unaffected by these developments. Although regular once-a-day boat service to Mo and two daily buses to the nearby railroad station are available, and a few people commute to Mo by private auto or motorcycle, for the bulk of local residents, life centers in and around the home town. Our interviews showed, for example, that events in Mo-i-Rana or its neighboring small towns are only of marginal interest to them.

Economically the settlement at Hemnesberget depends on district forestry. Production centers largely around the use of fairly cheap timber from the surrounding district in the form of boat- and ship-building. In addition there is a sawmill, two fairly recent furniture plants producing doors and window-frames, and another new plant which builds pleasure boats for international export.

Social life in Hemnesberget shows a fluidity of class structure similar to that described by Barnes (1954) for Southern Norway. The bulk of the population stems from similar culture backgrounds, and shows a strong sense of identification with the local community. There are, however, some significant internal distinctions. We distinguish four socio-economic groups: 1) artisans (who may or may not operate their own shops) and workmen; 2) shopkeepers engaging in purely local retail trade; 3) wholesale-retail merchants who buy and sell locally produced boats, and furnish supplies and general provisions to farmers and fishermen; and 4) government officials and factory managers.

We will use the term VERBAL REPERTOIRE to indicate the totality of speech forms employed by residents of Hemnes in socially significant linguistic interaction. We may visualize this repertoire as a range of lexical, phonological and grammatical superposed variants from which speakers select in accordance with the rules of linguistic etiquette current in Hemnes. At one extreme of this range, we find forms that are ordinarily identified as part of the local dialect, *Rana målet*. This is the native tongue transmitted from parents to children and generally employed in neighborhood play and friendship groups. At the other end of the range is located the standard North Norwegian variety of *bokmål* or *riksmål*, to use its older name. *Riksmål* or *bokmål*, one of the two officially recognized standard languages of Norway (Haugen 1959), is generally accepted as the only standard in Northern Norway. Children learn it in school and in church and are regularly exposed to it through radio broadcasts. Since

education is universal and residents are highly literate, *bokmål* may be considered an integral part of community linguistic interaction.

Although *bokmål* is recognized as the literary and religious language, villagers take considerable pride in their dialect as a spoken medium. As in many Norwegian rural areas, they are proud of their local heritage, and the dialect symbolizes this local pride. To use *bokmål* in a local context would be considered rude and 'putting on airs'. Many residents even claim to use their local dialect in their dealings in the city, to show, as they put it, that "we are not ashamed of our origin".

The grammatical differences between *bokmål* and *Rana målet* are such that the two are ordinarily considered as separate entities by linguists. Contrastive analysis, however, also reveals considerable similarities. Analyzed within the same frame according to similar criteria, they provide a scale for the measurement of linguistic interaction. As detailed contrastive analysis is beyond the scope of this paper, we list here only some of the most important phonological and morphological indices of this internal differentiation.

On the level of phonology, the shared and non-shared features of the verbal repertoire can be represented by distinguishing between the maximum phonemic inventory and the common core. Phonemic distinctions in the common core are found in all local speech varieties. Others (marked here by an asterisk) appear only in some but not all varieties. Only rough phonetic correlates are given below; additional phonetic detail will be cited in the discussion of verbal behavior.

The vowel system distinguishes three tongue heights. High vowels are front unrounded /i/, front rounded /y/, centralized back /u/, retracted back /o/. Mid vowels are front unrounded /e/, front rounded /ø/, back /ɔ/. Low vowels are front unrounded /æ/, front rounded /ɤ\*/, back /a/.

There are two series of consonants, unmarked and palatalized. Unmarked consonants include stops /p, b, t, d, k, g/; spirants /f, v, s, š, j, ç/; nasals /m, n, ŋ/; trill /r/, lateral /l/, flap /l/. The palatal series contains /tj\*, dj\*, nj\*, lj\*/. On the phonetic level, furthermore, a set of cacuminal or retroflex stop or spirant allophones occur after /r/.

In the realm of morphology, the basic inflectional categories are common to all varieties. Thus, for example, all nouns appear in an indefinite form consisting of the stem, and a definite form made up of stem plus suffixed article, both of which are inflected for singular and plural. There are three genders, masculine, feminine and neuter. In the case of verbs and verbal derivatives, inflectional morphemes distinguish imperative, infinitive, present, past and past participle forms. The pronoun system includes personal, possessive, demonstrative, interrogative, relative and indefinite pronouns.

Morphological differences among local speech varieties do not affect the grammatical categories themselves, but rather what we may call the morphophonemic realization of shared morphemes. (In what follows, dialect forms are marked by R and *bokmål* forms by B.) In some cases regular correspondences can be set up.

Final B *sk* may appear as R *sk* or R *š* before front vowels of suffixes, e.g. *fesk* 'a fish' (indefinite), *fesken* or *fešen* 'the fish' (definite). B /e/ and /i/ are often lowered to R /æ/ and /e/ as in *men* vs. *mæn* 'but', or *til* vs. *tel*, 'to, towards'.

In other cases differences affect the entire phonemic realization of the morpheme. Thus with nouns such as *hæst* 'horse', the plural allomorph is R *-a*, B *-er*. The present tense form of the verb 'to come' appears as R *gæm*, B *komer*. The stem of the verb 'to do' is R *jæ:r*, *jö:r*.

Differences are especially numerous with pronouns and with commonly used function words (prepositions, conjunctions, adverbs of place, etc.). Some examples are listed below:

B	R	I
je	eg	me
mej	meg	you (pl.)
dere	døk	he
han	hanj	who
vem	kem	what
va	ke	how
vordan	ke...lesn	to, towards,
til	tel	from
fra	ifrø	in between
mellom	imelja	

The formal study of verbal behavior was preceded by intensive work with a few informants and by observation of verbal interaction in a variety of social situations. Both superposed and dialectal variants were noted, but as had also been observed elsewhere (Hill 1961, Gumperz 1962), the two were found to have different linguistic correlates. The difference is most striking on the level of phonology. Here superposed variation is chiefly revealed by the presence or absence of specific contrasts, such as the distinction between palatalized and non-palatalized consonants. Palatalization tended to be dropped in formal interviews with strangers as well as in some business transactions. The phonetic realization or allophonic range (i.e., the range of free variation within which all incidences of an allophone fall) of particular phonemes, however, was not subject to such variation. Individuals used the same phonetics regardless of whether they were talking *bokmål* or dialect.

Dialectal variations among different individuals, on the other hand, were marked chiefly by phonetic differences. A rough picture of the distribution of such variants was obtained by surveying the speech of a small sample of informants which included residents of outlying farming districts as well as Hemnes residents of different age groups. The questionnaire contained a set of one-syllable words arranged in order of phonological structure along the pattern of those employed to test differences in allophonic range. A second set of words and inflected forms which are commonly regarded as antiquated was also included.

Some systematic dialectal differences were observed which divide the population into approximately two groups: 1) residents of rural areas and older Hemnes residents (average age about 60); 2) middle-aged and younger Hemnes residents. With the first group, allophones of vowels /i, e, y, o, u, ö/ tended to be lower and those of /æ, a, ɔ/ very low and retracted. With group 2, allophones were generally higher and less retracted. The difference was especially pronounced in the case of /æ/ and /a/, which appeared as [a<sup>ɪ</sup>] and [ɒ] in the speech of group 1 and as [æ̃] and [ɑ] in that of group 2. Furthermore, while both groups make the contrast between palatalized and non-palatalized consonants, with group 2 palatalization is less strong, hence the phonetic distance between contrasting palatalized and non-palatalized allophones is considerably lessened.

Antiquated features covered in the survey included the contrast between /ö/ and /ɔ/, e.g., *lõ:sa* 'lice', *hø:sa* 'socks', the variation between *-sk* and *-sj* mentioned above, and a number of older inflected verb forms, e.g. *ti:çe* instead of *tat* as the past participle of the verb 'to take'. These last proved difficult to elicit by indirect means and information on them was obtained mainly through direct questions. The /ö/ — /ɔ/ contrast seems to remain only as a memory contrast. Older informants recalled it when questioned, but no one was heard to produce it spontaneously. The *-sj* forms were heard with one older informant; others recalled them only upon suggestion. It would seem that all these items are on their way out, at least in Hemnes itself, and are being replaced by others closer to *bokmål*.

While dialectal variation as defined above can be studied through methods of linguistic geography, we have, as yet, no techniques for the formal analysis of superposed variation. Bernstein has shown (1961) that different subgroups within the same community have different attitudes to language use; and we assume that these attitudinal differences are also reflected in superposed variation. But the definition of significant subgroups on which to base our study presents some problems. Terms such as class rarely correlate with any single measurable index, especially in communities with a fluid class structure such as Hemnes. Since we are measuring language usage rather than ability to speak, there is also no theoretical reason why such indices as socio-economic status and education should be directly relevant in any case. Many relatively poor and uninfluential Hemnes residents are considerably more sophisticated than their wealthier compatriots, while some educated people readily adopt the behavior patterns of their less educated peers. In determining the indices to be employed, we viewed superposed variation as a way of symbolizing different social relationships. It follows, therefore, that this type of variation must be studied by means of the interaction patterns through which social relationships are maintained. This can be accomplished through the concept of social network (Barnes 1954).

A network is basically a group of people who know each other. We can distinguish further, however, between CLOSED and OPEN networks. Given three individuals A, B, and C, if A and B are acquainted, and B and C are also acquainted, the network is CLOSED if there is a high probability that A also knows C. If, on the other hand, there

is a high probability that A will not know C, the network is said to be OPEN. Networks may be based on various kinds of ties: friendship, religion, politics, kinship, commerce, occupation, etc. Since a previous study of social dialects in India (Gumperz 1958) has shown that linguistic similarity is most closely reflected through friendship ties, we based our analysis on friendship networks. These were isolated in a separate study by a Norwegian anthropologist (Blom 1964). Hemnesberget residents of socio-economic group one (artisans and workmen) were found to be largely part of closed networks; that is, they tended to form their friendships mainly within the community. Many members of groups two, three and four, however, showed open network characteristics and had significant personal relationships both within and without the community. Our study included four discussion groups, two made up of individuals showing open network characteristics and two with participants showing closed network characteristics. Open network group *a* consisted of University students between the ages of 20 and 30, who had spent the winter studying elsewhere and had returned to Hemnes on summer jobs. Open network group *b* included a local businessman, his wife, and a clerical employee of the commune. Closed network group *c* included a local businessman and two foremen of small industrial establishments with their wives. Closed network group *d* consisted of two skilled craftsmen and their wives. One of the participants in group *c* was an elected member of the town council; the other, a past member, was still active in local politics.

Since superposed variation reflects interpersonal relationships in particular settings, questionnaire data collecting techniques could hardly be effective, for these tend to cut off individuals from their normal surroundings and from the stimuli that motivate the kind of behavior under study. Superposed variation is best studied by direct observation of groups in particular interaction settings. By varying the settings and the network characteristics of groups under study, a variety of test situations for the study of verbal behavior can be created.

Settings can be differentiated further with respect to the type of interaction predominant in them. One type of interaction, which we will call TRANSACTIONAL, centers largely around certain limited goals such as purchasing such items as groceries or clothing, cashing a check at the bank, getting the telephone operator to put a call through, going to the doctor. Participants in such interactions in a sense suspend their individuality and act by virtue of their status, in the sense in which this word is used in social anthropology (Nadel 1954). They act as salesmen, customers, bank tellers, physicians, rather than as Tom Hansen or Inger Stensen. Each society has definite norms of behavioral and linguistic etiquette which attach to these statuses. Regardless of their individual personalities, occupants of such statuses are expected to conform to these norms. An individual may, of course, occupy several statuses in the course of a single day; he may be a minister, a father, a bank customer, etc. In each case he signals his status by his behavior, his dress, his posture, and his speech style. To deviate from the expected behavior in each of these statuses would be to risk defeating the goals of the interaction.

Transactional interaction contrasts with PERSONAL interaction. Participants in the latter act as individuals rather than for the sake of limited, readily apparent goals. This kind of behavior predominates among friends, within peer groups, and within the family circle in periods of relaxation. It gives scope to all the facets of an individual's personality.

Both types of interaction may be marked by superposed variation. In transactional interaction, superposed variation tends to be closely related to the formal characteristics of the setting. Changes in style may result from a change in the physical environment; they may be due to a change in the interlocutors, as when a new person joins the group. Or they may reflect an alteration in the structural characteristics of the situation, as, for example, when a shopkeeper intersperses his business dealings with a customer with friendly chatter. In so doing, he calls upon a personal relationship which is separate from the transactional character of the main interaction, thus temporarily suspending the goals of the transaction. If this personal relationship does not in fact exist, he risks misunderstanding. In personal interaction, on the other hand, superposed variation can be elicited by a change in topic, although the general characteristics of the situation remain the same. We use the terms TRANSACTIONAL SWITCHING and PERSONAL SWITCHING to distinguish the stylistic shifts relating to the two types of interaction.

We assume that since all Hemnes residents are bilingual, they all engage in transactional switching. This assumption is confirmed, in part, by observation of speaking behavior in stores and public offices. Here, both closed and open network members seemed to show a significantly higher incidence of *bokmål* forms and pronunciations. A similarly high incidence of these forms was observed when participants in the group discussions reported on below were interviewed separately under conditions of relative formality. Since this was the case, we then looked to personal interaction for inter-group differences in language usage.

The following procedure was used in eliciting the linguistic data for the study of superposed variation. All of the four groups listed above were observed in a single type of setting: friendly evening gatherings over a glass of beer. Both investigators participated in the gathering. Meetings with each group took the better part of an evening, and several hours of conversation were taped in each case.

Each group was exposed to similar topical stimuli, ranging from relatively abstract themes regarding government investment policy, local administrative problems, and the like, to more informal subjects, fishing, excursions, personal foibles of well-known members of the community, etc. Group members were told that we were interested in natural speech and were encouraged to speak freely. Topics were introduced by one of the investigators, who would begin, for example, by inquiring about good fishing spots. Initially, answers would be addressed directly to him, but if the topic proved productive, participants would soon become involved in internal discussions of their own. After a sufficiently long passage of internal conversation had been recorded, a new topic would be introduced.



After each discussion session, tapes were edited to eliminate those passages which were addressed directly to the investigator. Since these passages reflected the investigators' status as strangers, they could be considered transactional. Elimination of such passages proved relatively easy with the aid of visual clues noted during the discussions. In any case, the shifts from transactional to personal passages were signalled linguistically by marked increase in sentence speed and by the ease with which conversation flowed.

Analysis of the linguistic results of these meetings is still in progress; a detailed report will be presented at a later date. Our preliminary evidence, however, indicates some significant differences in verbal behavior among groups of different network characteristics. All four groups tended to confine themselves to dialect forms when the topic dealt with such matters as sports and local gossip. When the conversation turned to more abstract issues of other than purely local reference, open network groups tended to show a much higher incidence of *bokmål* forms. Personal switching in these instances affected both the lexicon and the linguistic structure. *Bokmål* terms and expressions required to convey concepts such as government investment or labor problems would tend to co-occur with *bokmål* pronunciations, inflectional suffixes and function words. There would thus be a higher incidence of unpalatalized forms, e.g. *han* (R *hanj*) 'he'; of *bokmål* prepositions and adverbs, e.g. *mellom* (R *melja*) 'between'; and of plural endings in *-er* (R *-a*).

Our evidence further indicates that personal switching in open network groups is independent of expressed attitudes to language. Some members of such groups, if anything, tend to be even more strongly in favor of using the dialect for all types of spoken interaction than their compatriots in closed network groups. When tapes of one open network discussion were played to a member of the second open network group, he first claimed that participants were not Hemnes residents. Then, recognizing individual voices, he registered disapproval. These same tapes were afterwards played back to one of the discussion participants, who expressed surprise and stated that she did not realize that she had been switching to this extent. She expressed her intention to avoid extensive switching in the future. Nevertheless, tapes of subsequent discussions involving this individual showed little change.

#### CONCLUSIONS

This analysis of verbal behavior is brief and exploratory rather than definitive, but the results seem to indicate that the study of change based upon analytical distinctions in types of verbal behavior allows us to develop reproducible techniques which may be tested in subsequent studies. If our finding that differences in allophonic range are a product of personal history is confirmed, it would make possible the development of questionnaire methods focussing on pronunciation which could be administered with a minimum of contextual controls.

The measurement of superposed variation in terms of networks allows us to dispense with concepts such as 'class', 'prestige', and the like, which are notoriously difficult to define in particular communities and to relate to behavior in specific contexts.

Turning to the broader implications of the study for linguistic change, it would seem that dialectal variation and intergroup differences in superposed variation indicate two different but simultaneous on-going processes within the same community. Dialectal variation reflects a long-term gradual adaptation in speech habits. The trends observed in Hemnes lead us to predict a gradual reduction of phonetic differences between the dialect and the standard, accompanied by gradual assimilation of dialect grammatical forms to the standard. Specific symbols of separateness may be maintained, but the total language distance appears to be decreasing.

While dialectal variation relates to 'drift', or linguistic change in its more common sense, superposed variation relates to 'language shift' and borrowing. Its linguistic effect is the replacement of whole words. Such replacements may eventually cause the loss of phonemic contrasts or of certain allomorphs by decreasing the incidence of features characteristic of the dialect.

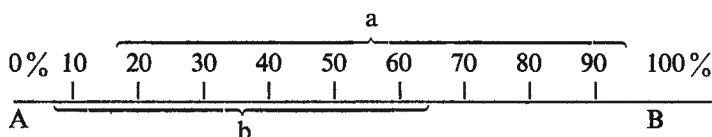
Our distinction between transactional and personal switching points the way toward a solution of the problem posed by Emeneau, namely, the persistence of bilingualism in some communities and not in others. Studies in historical linguistics and in bilingualism would lead us to hypothesize that personal switching is more likely to lead to language shift than transactional switching. The more frequently two alternatives co-occur within the same context, the greater the tendency for the connotational difference between them to be lost. When this occurs, the social reason for their separate existence is lost. Transactional switching, on the other hand, tends to reinforce connotational differences by relating them to overt situational clues. If the hypothesis is valid, examination of the social characteristics of groups which favor personal switching could produce predictions of language shift. In Hemnes, personal switching is confined to open network groups. Our ethnographic analysis shows that these groups are marginal to the community as a whole. We would therefore assume that, aside from gradual trends discussed above, the dialect is relatively stable. If, on the other hand, personal switching is carried on in both open and closed network groups within the same community, we would conclude that the verbal behavior of the community is undergoing change. Studies of verbal behavior in communities showing various kinds of social stratification would confirm this hypothesis.<sup>1</sup>

<sup>1</sup> This paper is the result of a coöperative study made by the author and Jan-Petter Blom, Lektor in Anthropology at the University of Bergen, Norway. Mr. Blom is responsible for the independent study of friendship networks in Hemnes. He assisted in the group discussions and provided much valuable help in formulating details of the research design. — The study was sponsored by the Institute of Sociology, University of Oslo. Thanks are due to the Director of that Institute, Professor Sverre Holm, for personal encouragement and financial assistance, and to Dr. Hallfrid Christiansen of the University of Oslo for making available her excellent grammar on the Rana dialect. — The author's stay in Norway was made possible through a grant from the National Science Foundation. I am also grateful to Mr. Basil Bernstein of the Institute of Education, London University, for stimulating discussions which greatly aided in the general formulation of the research design.

## DISCUSSION

GARFINKEL: I would like to add to Dr. Gumperz' presentation from an unlikely source. My colleague, Harvey Sacks, and I have been spending this year at the Los Angeles Suicide Prevention Center. Among other things, we have been interested in what people are doing when they call in over the phone. An important part of the Center's work is done over the phone. Your remarks bring to mind something that occurs in many phone calls between Center personnel and clients. Both parties quickly fall into address that is appropriate to dealing with an agency. That holds for a person who would seem to be suffering the most godforsaken experience of his life. For example, a sixty-year-old woman who could barely drag out her remarks began by saying, "Is this the right place to be calling?" The social worker answered, "Yes, it is." With that exchange the rest took off, as if the transaction were the method whereby the client received the information, "You need have no fear; we are here to take care of you. You are talking in the right way to get done whatever it is you figure you need to get done."

P. Ivić: In the communities where both a local dialect and a standard language are spoken, most of the people have a range of mastery, embracing a good part of the distance between an ideal of standard language and an ideal of local dialect. These ranges are combined with a certain ability of code-switching and vary as to personal history. They could be presented this way:



If *A* is 100% local dialect and *B* 100% standard language, various persons would have ranges such as *a* or *b*. The first few percent and the last few percent of the diagram would be the least populated, because almost nobody in such places masters the literary language perfectly; and on the other hand only the most backward people, the least cultivated among the oldest ones, preserve the pure form of the dialect. The differences concern of course various parts of the linguistic pattern, and the way how people adopt elements of the standard language can be analyzed structurally. Things which are the easiest to adopt are differences in the incidence of existing elements. For instance, if the phonemes /a/ and /u/ occur both in the standard language and the dialect, but the standard form of a certain word is /pat/ whereas the dialect has /put/, one of the first achievements of the speakers toward the mastery of the standard language would be to adopt forms such as /pat/. The second stage is introducing phonemes which are not usual in the dialect, whereas additional distinctive phonemic features enter the pattern only at the end of the development. Thus, the whole process of adopting the standard language can be envisaged as a sequence of structural steps. On the other hand, there is a process of abandoning distinctions which exist in the dialect

but not in the standard language. Those distinctions are much more easily abandoned than the other distinctions are learned. What prevails is a kind of simplicity which can be described as a reduction of the number of distinctions.

LABOV: Many of the comments that would be appropriate for me to make on Dr. Gumperz' paper would best be made in the course of my own presentation, but there are certain things that I would like to say now. I am able to profit by this paper because I have been wrestling with many of the same problems, and also because the methods he is using are complementary to those that I'm using; I am conscious of the holes in my approach to language in an area which he has filled. In the course of our own work in New York City, a great deal of our effort has been concentrated on the problem of compensating for the fact that in the formal linguistic interview you do not get the most natural linguistic situation — the important one for the genetic development of the language. It is essentially the small group situation which is most real as far as language development is concerned. Anything else is one stage removed from reality. And therefore, as Gumperz explained the highly systematic nature of this two-dimensional type of shift, both transactional and personal shifting, it was extremely encouraging, because in this natural situation he confirms the analysis which we have made by indirect means.

Dr. Gumperz presents the setting up of this group situation in an offhand way, as if to say, "Well, it's very easy; just call them in and that's all there is to it." I know there's a great deal of skill in this — I know this because I failed signally when I tried it on Martha's Vineyard. On my last trip I suggested to one of the community leaders that he call together a lot of his friends, his brothers, his cousins, and we have an evening together. Everyone said, "Fine, fine." But one by one the people made excuses and nobody showed up. Why? It appeared that all of these informants had earlier conversations with me which they enjoyed very much but which led to topics in which they expressed themselves very strongly about their neighbors. They said things to me quite freely, as a stranger, that they would never dare say in front of their friends. Now, if I had foreseen this at the beginning, I never would have gotten into topics which were so vital to the life of the community. I am therefore in a good position to appreciate the amount of forethought and skill needed in the Norwegian work: one wrong step in the community would have made the whole program impossible.

Another problem concerns the degree of complexity of the community. I was dealing with a most complex society in New York City, compared to the community that you picked, and therefore this comparison helps to answer the question, "Are we dealing with processes and reactions which are peculiar to a particular community, or are we getting at procedures and linguistic developments which may apply generally to the history of languages?"

FISCHER: I am very interested in this distinction between transactional shifting and personal shifting, and I think that what you say about it is correct. Where you get these personal shifts, you have a less stable situation. On the other hand, it seems to me that you imply that this should eventually reach complete homogeneity. Is that

the case wherever you have a personal shift involved? Do you foresee that all shifts correlated with topics should eventually disappear, if you could isolate this community from others?

GUMPERZ: No, I would say the opposite. I would say that personal shifting would lead to homogeneity, but only with respect to those forms that are subject to shifting — that new forms must be created elsewhere. As a matter of fact, I think you could show that in a system of linguistic communication, you must have a balance between formal situations, where you have transactional shifting, and informal situations where you can change and do topical shifting. I think both must exist for communication to be effective.

BRIGHT: Do you have any ideas about how brand new forms — words or pronunciations or whatever — come into a community such as the one you studied?

GUMPERZ: Well, actually there are two kinds of things. There were one or two changes in the local dialect which I couldn't explain. They were simply inversions of the normal word order which seemed to have arisen in neighborhood play groups. You simply inverted the word order of a particular construction, and that seemed to be characteristic of that play group conception. Then it was taken over into the community as a whole. The MAJOR source of innovation, of course, is standard Norwegian, and English through standard Norwegian. And you CAN trace broad developments there, but I would hesitate to generalize from one small group pattern to another small group pattern. You see, if we have two neighboring villages, and trend A exists in the first village and trend B in the second village, the two may balance each other out. If you are talking at a greater level of generalization, you can't really make any general statement. So I think that one of our problems, the problem Dr. Labov mentions, is how do you generalize from this kind of a small group study? I think a small group study gives you an insight into the processes, but I think you need a large scale survey in order to get the broader trend.

SAMARIN: I'd like to see a similar study made in a bilingual community where there is a great deal of interference, like the large Russian-speaking community here in East Los Angeles. In the casual speech of these Molokans, it is hard to say where English stops and Russian begins. But in a more formal situation — in their church doings for example — there is no mixture of languages whatsoever. This purification happened too, as I remember, when my father used to speak to me more formally, a young person would say 'preached' to me. A similar thing happens among the younger generation, that is, those who learned Russian after they were teenagers as part of their return to the traditional culture. I don't think that they can carry on a conversation with the older generation, and they don't seem to ever try to with their peers. I would say that they could not really talk Russian. But at religious functions they can give really creditable discourses which will last for several minutes. In other words, on one level their grasp of Russian is very inferior, but on another level, in a different situation — a different transactional context — they speak good Russian. I suggest that when there is a peer-transactional difference, there is not going to be very much

interference, and I think the East Los Angeles Russian situation bears this out. And perhaps this formal situation is going to influence the casual Russian of the young people in such a way that gradually their informal Russian is going to be better. But perhaps the speech situations are going to remain so distinct that their fluency in one area is not going to affect their performance in another. In any case, the Molokan community would be a good laboratory for studying the kind of thing we're talking about right now.

BRIGHT: As another example, in field work on American Indian languages I have found some informants who spoke their Indian language fluently among themselves; but when I asked them to work with me, they simply became tongue-tied, and with all the good will in the world they simply could not speak their language coherently anymore.

HAUGEN: Dr. Gumperz' paper interests me very much for a number of reasons. For one thing, it involves the total linguistic situation in Norway, which does affect to some extent the people he has studied. This total situation is one which I have been writing a book about for the past nine months and therefore have a direct interest in. On the other hand, this paper also comes close to me because the dialect that he has here studied is one which is not too different from the one that I myself learned as a boy from my parents and from two years of residence in a community in Norway. I still speak it, and have done field work on it, with the intention of sooner or later producing a monograph on the dialect. It has virtually the same phonemic system as the one presented by Gumperz. In the course of my work in the dialect, I found that I was unable to give a monolithic description of it because there were numerous internal variations among the speakers whom I interviewed, even though the total number of speakers of the dialect does not amount to more than three or four thousand persons. This led me to go back in the field and do a cross-survey of the whole area in terms both of its geography and of its age groups. I worked out a grid with ten geographical sections horizontally across the top of the board, each section being a school district. The children who went to the same school presumably would constitute a kind of subgroup within the dialect, and there proved to be 10 such school districts within the area. Perpendicularly on my grid I placed the generations by decades. Since the number of people involved covered about 10 decades, I then had 10 squares in each direction, and not quite enough people to fill all of these squares since I had about 50 informants. I found that there was an extremely interesting gradation within this grid in both directions, and that it was possible on the basis of the grid at least to identify — I can't say explain because this would involve an historical explanation which I'm not prepared to give yet — but at least to identify some of the features which Dr. Gumperz has indicated as dividing speakers within the community from one another, and which are part, so to speak, of a person's identity as an individual speaker of the language. Call it idiolect, or whatever term you choose to identify such a situation. There was a clear difference between the oldest speakers and the youngest speakers, in which the youngest speakers gradually were approaching some

form of the standard. I even found some children of around 8 or 9 who had abandoned one of the pillars of the dialect, namely the use of the dative case. This shocked me deeply, needless to say, and gave me a sense that perhaps in another generation the dative and other features which characterize this dialect will go by the board.

However, there is a countermovement to this which I think is important, and which may be significant in our sociolinguistic discussion. It was pointed out to me by scholars in Norway that even if the children deviate from the dialect toward some kind of standard form, they are not necessarily going to continue this form of the language when they grow up. They may grow into the adults' form of language — the middle-aged man's form of language, which will thus carry across generations. Some years back Hockett wrote an article on age-grading, in which he implied that one could predict linguistic change purely by taking a generational or age-wise cross-section of a group of people. What the children were saying would be the language of the future. To some extent, of course, there is a drift in this direction, but we have to recognize that there is also a countermovement across generations.

Then, one final point. The particular phonemic contrast which Gumperz chose to use as a shibboleth, the difference between a non-palatalized and palatalized set of consonants, is an excellent choice. The particular reason is that while it definitely belongs in the dialect, it has a weaker phonemic status than many other oppositions. It is not popular to speak of 1½ phonemes. Phonemes are conceived of as either-or — either it is a phoneme, or by golly, it isn't a phoneme. This attitude doesn't work well when you are dealing with groups of this kind in the field. As we all recognize, there are certain phonemic contrasts that are less heavily burdened than others, and this is one of them. It's rather easy to get rid of it — you alter nothing in the dialect by dropping it completely, because it is difficult to find minimal pairs for contrast. They will usually include loan words. In the dialect that I speak, a typical difference is that between the name *Anna* which is pronounced /an'na/, and the word that means 'other, something else' — Old Norse *annat*, which is pronounced /aŋ`ŋa/. *Anna* and *annja* are about as close as one gets to a minimal pair, and one of these is really a foreign word, with a different tonal pattern. Dropping the palatalization will not disturb communication, and serves to mark one's rejection of the most extreme forms of the dialect.

GUMPERZ: One reason I like to speak of verbal repertoires rather than of languages is because of the kind of forces that Dr. Haugen mentions — the pressures on the part of the adult community for conformity with their own value system — and the value system in Norway, by the way, is toward the dialect. People say, "We are from Hemnes. We speak Hemnes dialect wherever we are." As a matter of fact, they were very upset when we played back the tapes and found they were shifting. Our best informant, who is also trained as a philologist, refused to transcribe the tapes — he said, "I couldn't possibly do this. It would upset me too much."

But the general trend, if you describe the difference between styles in terms of rules, is towards a reduction in the number of rules needed to translate one into the other.

Although certain phonetic things remain different, the tendency is to reduce the totality of the distance. Language distance is decreasing although overt symbols of differentiation are remaining.

KELLEY: You mentioned the north Indian non-Brahmin power elite and their lip service to the Brahmin. Would that extend to lip service to his usage, and if so, couldn't you expect some transactional shifting? I think this would happen in South India.

GUMPERZ: Well, I think you could. If the Rajput were to engage in a public lecture, he would use certain Brahmin forms — certain Sanskritized Hindi forms. This would be the appropriate style. But if the Rajput were to engage in, let's say, a private discussion on a serious topic, he would be likely to use Anglicized forms instead.

KELLEY: Doesn't he feel this self-image insecurity about his own image?

GUMPERZ: Oh no. And in terms of long-term trends, I would hesitate to predict that Rajput informal speech would change in the direction of Brahmin speech.

KELLEY: Well, there would seem to be quite a different pattern in South India, in the Telugu area and particularly in Telengana, where the power structure is non-Brahmin, there still is insecurity about one's own usage, particularly in face-to-face contact.

SJOBERG: Prof. Gumperz has mentioned in his paper, almost in passing, a phenomenon that I think needs a lot more investigation. I refer to the situation where a 'minority' language is not displaced as a result of prestige imitation. He mentions the example of three Dravidian groups that have lived in a 'caste-like' relationship for hundreds of years, with the Toda at the top and the Kota and Badaga performing services for them, without the latter groups giving up their own languages. I think that what needs to be investigated here is the kind of social system involved. The Todas at the top do not constitute the leaders of a nation-state system, nor does their language even have a regional official status as does Kannada or Telugu, for example; and from what I gather, the services the other two groups perform are part of a somewhat 'symbiotic' relationship: these people undergo no real political domination by the Toda. Moreover, there is no standardized form of Toda — and in the absence of interbreeding between the groups involved, the Toda language would have to have a literary tradition if it were going to spread to these other peoples and actually REPLACE the latter's own languages. The absence of writing and therefore of a standardized form of the language, the lack of formal education in Toda and of political dominance by this group, all combine to account for this failure of the 'minority' languages to disappear.

GUMPERZ: Well, I think there is evidence in the ethnographic literature that languages disappear even where there is no writing system. There is a paper by Frederick Barth, a Norwegian anthropologist (1964), where he studies the language shift between Baluchis and Pathans. Some Baluchis and Pathans are literate in Urdu or Pashtu, but they are not in their own dialects. Barth finds that the Pathans are the



dominant group politically, while the Baluchis are poor peasants; but nevertheless, Baluchi is spreading with respect to Pashtu. He explains this not in terms of a national political set-up, but in terms of small-group power relations, and he shows exactly how a shift in power relations can lead to a shift in language usage. I think in the broader sense writing is very important, but it becomes important at a higher level.

FERGUSON: I was very much interested in the hypothesis that was put forward about the stability or relative instability of personal variations vs. transactional variations. The first thought that occurred to me was that in Arabic, probably for centuries, there has been shifting by topic toward Classical, and this has remained a stable kind of shifting: if you talk about certain religious or cultural topics, you would move in the direction of Classical vocabulary, certainly, but also of Classical grammar and certain phonological features. The other aspect which struck me as not being covered at all in this picture had to do with the relatively homogeneous nature of the speech community. The people who were talking dialect all had about the same kind of dialect. Now, very often there is linguistic interaction among speakers of fairly different kinds of dialect; and what is more, there are patterned shifts which take place there, too — transactional shifts of one sort or another. One study that has been done in Arabic by Haim Blanc showed that when a group of informants from different dialect areas sat together and talked about certain topics, they had these two kinds of shifting: one kind toward Classical, because they were talking about educated kinds of topics, etc., and the other toward a certain kind of common denominator dialect. That is, dialect speakers, regularly and in a patterned way, eliminated certain dialect features because they felt that they somehow stamped them as belonging to a particular dialect group. And this is a kind of transactional shifting, if you will, which didn't seem to be covered in the framework that you described.

Also, certainly in the Arab world and I assume in other language situations, too, there are cases where there is transactional shifting of a much more limited sort. For instance, when a Bedouin speaks Bedouin Arabic to a sedentary speaker who speaks sedentary Arabic, and they understand each other, this is appropriate for that particular situation, and only a moderate amount of shifting takes place. I like the idea of personal versus transactional, but it seems to me you have to expand it a little bit to take care of some other kinds of situations. I wonder whether this hypothesis is so promising or suggestive in areas which aren't moving in some sense toward homogenization on the basis of a standard.

GUMPERZ: One of the problems with this kind of a study is in our definition of terms that we draw from social science. What we are doing at this stage is throwing out hypotheses, and we know too little to generalize — but I think that one problem that affects most linguistic studies is that they are not based on formal analyses of social systems of the type that a social anthropologist makes. When a social anthropologist talks about status, for example, he tries to separate status from individuals. Status is something that relates to rights and duties and obligations. In other words, a person is a shopkeeper in the morning or during the day, and a father

in the evening. His wife may walk into the store and they can talk about business matters and he will use the shopkeeper's style; they shift, and he will assume a personal style. The problem is, to find some occupational correlates to predict this kind of shift. What I'd like to study is: When Arabs shift from the standard to the local dialect, do they also shift in sentence speed? Do they also shift in various kinds of contours? We need to take in much more of the whole message to find out, and I think the clearest way to distinguish between transactional and personal shifting is this matter of sentence speed; hesitation pauses, such as Freda Goldman Eisner has studied, are very important. Transactional talk seems to have a higher amount of hesitation pauses. So I think we have to broaden our range of linguistic investigation on the one hand, and also we have to sharpen our concepts of status and of defining the groups that we are studying.

LABOV: We have found it useful to divide linguistic behavior into two parts. We isolated tempo, volume, pitch, etc., from the segmental dependent variables that we were studying in order to give us an additional dimension of definition of casual style, which is just what you are suggesting. If we had used only a contextual situation to define casual style, the results would not have been as precise as in converging upon casual style from two different directions. The problem was to avoid using any cues which are structurally interrelated with the dependent variables. Our assumption was that tempo is not structurally interrelated with any of the segmental variables that we were using.

There is a separate question I would like to ask concerning sampling. It seems that we are going to proceed for a while with two different techniques — sampling small groups, and also sampling individuals on a larger scale. In the United States I have been trying to reduce the size of the sample, starting with the size appropriate for a sociological survey, and then since linguistic behavior is indeed more general than the average type of social behavior, to cut down the sample. Our next step is to take groups of informants in one interview, and deal with them as small groups. Now I have raised the question without much success with social workers, "We want to sample the groups in the area. How do we go about sampling small groups?" — the idea being that we don't want to take just one small group, we want to take a representative number of small groups. Have you thought about what you would do if you went into Oslo and you wanted to do the same thing on a larger scale with some kind of cross-section of interacting groups?

GUMPERZ: Well, I would still want to take care of both my situational variables and my groups, and I think I would define my small group not by any outside criterion but by an operational criterion such as friendship. This is something like what a sociometrist does, asking, "Who are your best friends?" I would try to do a typical small group study of this kind. What I'd like to see done is to take a larger area, like a city, and have a series of planned deep studies made, and then a series of surveys made, and see how the predictions from the surveys correlate with the predictions that would derive from the small group study. I think the combination of the techniques is really

the interesting thing, because the processes must be the same; but what is wrong somehow is that we haven't gotten at the right things to measure, and the most productive things to generalize on. This we could get by this combination study. But the problem we are talking about is a problem that is a point of issue between anthropologists and sociologists at all times. The anthropologist studies small face-to-face communities, and as a South Asia specialist in my own university, I am always asked, "Now what relevance does your village work have to larger problems of economic development, etc.?" Sometimes I can't give an answer, simply because I haven't done this combination study.

HAUGEN: I wanted to add a comment about the bearing of all this on bilingual studies. Usually we think of bilingualism as involving the relations of two distinct languages. Here Gumperz has been dealing with what everyone agrees are really two distinct dialects, and yet the phenomena which he has detailed show that here we have some of the same things happening. Switching from one to the other of two languages means that the speakers have to have two psychological networks sufficiently distinct so that they can switch from one to the other. And this would be true for students who go away to the University and speak a kind of standard Norwegian there, and then come back to the community and speak the dialect. But apparently the closer the two get together the harder it is to keep them apart, because there is a much greater overlap. The distinction between the two becomes less and less important. It raises one of the old problems of linguistics, of the difference between a dialect and a language. It may be a pseudo-problem, because we are using terms that are essentially popular in meaning and trying to give them some kind of scientific relevance. Like most words, they are extremely ambiguous and their meaning depends on the particular context. The absolute border between dialect and language which Bloomfield established, that of mutual comprehensibility, is a weak one, because, as we know, there are degrees of comprehensibility. These people all comprehend almost completely the standard language which is thrown at them from all the mass media, so that when they talk about the subjects that are talked about in the newspapers, they have no other vocabulary than that which these give them. This is the source of the continual pressure on the dialect in a modern community where everyone is literate. Here we get a diminishing language distance, as long as the same phenomena keep occurring — loan words, switching, interference. As you get closer, things begin to look different, until eventually you get down to the differences between individuals. I think it is going to be quite a problem in sociolinguistics to set up adequate categories here that we all will accept, running all the way from the individual speech to that of a nation or of a world.

HOENIGSWALD: Conventional studies have always distinguished two characteristic processes — one is borrowing into a dialect which otherwise maintains its identity, the other is shifting to a prestige language. But apparently there's a critical point in similarity or intelligibility when it becomes difficult or perhaps meaningless to distinguish between prestige borrowings and prestige shiftings, and maybe this is the

critical point for us to make a distinction between dialect bilingualism and gross bilingualism. I wonder whether Dr. Gumperz has anything to contribute concerning this traditional distinction.

GUMPERZ: Well, I think as an operational assumption it is useful to keep our popular notions and our linguistic notions apart. Maybe we should leave 'language' as an undefined term and talk about verbal repertoires. This is where some of our folk-linguistics comes in. I think it is true about the history of linguistics, that so far we have studied either tribal languages, or we have studied western systems, highly urbanized western societies. What if modern linguistics had arisen in, let's say, the Near East or Greece or in modern India? Then we would have had to assume that the grammatical shifting was part of everyday linguistic phenomena. It seems to be a cultural difference that is involved rather than a linguistic difference, and I think that the linguistic phenomena in all of this, at least as an operational assumption, should always be our dependent variable. We should always look at what are the social facts that may cause a shift. Now, if you use the notion of the verbal repertoire, there's one way in which you can distinguish formally on a linguistic level between different kinds of situations. If you assume that linguistic interaction is a matter of choice, this choice then has two kinds of correlates. One is the social situation. But once you've chosen the form, you are always bound to certain co-occurrence rules. In other words, once you say, "I ain't", you can't say, "I ain't going to do this." You have to say, "I ain't gonna do this." In a bilingual situation, the co-occurrence restrictions are a lot stricter and a lot different linguistically than they are in the bi-dialectal situation and in the stylistic situation, where the shift is a matter of lexical items.

HYMES: I want to reinforce the importance of the starting point of this paper — the conception of verbal repertoire — in studying the linguistic habits of a community, because I think this is perhaps a more radical and subversive notion than is fully appreciated. The point is that Gumperz begins with a notion such as verbal repertoire, not with the notion of a language or a code. Here there is emerging what social linguistics must be if it becomes anything distinctive, rather a whole new approach to the description of language, which starts from the speech habits of a community as its frame of reference, and realizes that a speech community does not simply have a language, but a socio-linguistic system, a system of the repertoires of the individuals who are assigned social positions, of the situations in which the language can be used, and of the purposes for which it is used. This may be distinctive of an individual community and it is this system which governs all these other things we may study. If we are interested in the fundamentals of change, or anything else about the social aspect of language, we can extract particular processes from particular communities and compare them with a great deal of profit and interest. But fundamentally there is really a second descriptive linguistic science underlying the one with which we are more familiar — I've called it 'ethnography of speaking' — which is perhaps socio-linguistics. Whatever you want to call it, it consists of describing the community in these terms as the basis for the understanding of the rest.

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# LINGUISTICS AND LANGUAGE PLANNING

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## 1. INTRODUCTION

1.1. The problem of correctness in language has been a topic of absorbing interest to a number of writers in the United States for many years. The issue has arisen once more since the appearance of a third edition of Webster's *New International Dictionary*, hereafter referred to as Webster's Third. Some estheticians of the English language, chiefly self-appointed guardians of its purity, have associated the policies of Webster's Third with the name of structural linguistics. We are told by one notorious critic of Webster's Third, Mr. Dwight MacDonald (1962), that its lexical method is based on "the theory of Structural Linguistics". Now there is no evidence either in the editor's statements or in the practice of the dictionary to support this surprising claim. Nevertheless we are informed by Mr. MacDonald that the dictionary is an example of "the infiltration of Structural Linguistics into places it doesn't belong". If only to protect the good name of linguistics, I believe we should examine the relationship between the science of language and the problem of linguistic norms. Without attempting to refute the absurd accusations here made against structural linguistics, we shall try to formulate once again, for our generation, the nature of linguistic normalization and the potential role of the linguist in codifying norms and giving them the sanction of authority.<sup>1</sup>

1.2. Prior to the nineteenth century it is safe to say that ALL LINGUISTICS WAS NORMATIVE. The much-admired Pāṇini was a linguistic law-giver, whose work served the purposes of religious continuity. The Greek and Latin grammarians were textbook writers, who wished to establish immutable norms for the correct writing and speaking of their languages, the *ius et norma loquendi*. Perhaps this is the reason their work is regularly referred to in slighting terms as "pre-scientific". But even in the nineteenth century many of the distinguished founders of the new linguistic science were deeply involved in problems of normalization. The Danish Rasmus Rask spent a great deal of time in devising a more rational spelling for his native tongue and published a whole book on the subject (1826). German founders of the historical school of linguistics, like Jakob Grimm and August Schleicher, wrote extensively on the subject of correctness in German. In the second half of the century, when the Neo-

<sup>1</sup> This paper is a by-product of my work on language planning in Norway (cf. Haugen 1959, 1964).

Grammarians dominated linguistics, they too made important contributions to the problem. Hermann Paul devoted a whole chapter of his *Principien* to the "Gemeinsprache" or standard language (1886: 350-68), while Adolf Noreen wrote a penetrating study of the problem of correctness in language (1892). In England the great founder of phonetics, Henry Sweet, was active throughout life in the Spelling Reform Association. In the twentieth century Antoine Meillet considered the problem in great detail (1928), while Otto Jespersen devoted two full chapters of his *Mankind, Nation, and Individual* (1925) to the subject of "standards of correctness". Jespersen also took active part in the work for international auxiliary languages, creating one of his own called Novial.

1.3. In our own country two of the best-known founders of linguistics, Edward Sapir and Leonard Bloomfield, both took an interest in the subject. Sapir worked for the International Auxiliary Language Association (Sapir and Swadesh 1932). Bloomfield wrote an article on "Literate and illiterate speech" (1927) and devoted several pages in his book *Language* (1933) to the application of linguistic science to problems of correctness and standard language, as well as to those of English spelling and international languages. In his conclusion he wrote: "It is only a prospect, but not hopelessly remote, that the study of language may help us toward the understanding and control of human events" (Bloomfield 1933: 509). Even the anti-normative pronouncements of Robert A. Hall, Jr. represent a vigorous concern with the problems of a normative linguistics. Urging people to "leave their language alone" (Hall 1950) or to keep their "hands off pidgin English" (1955) is in itself an evaluation of competing policies with respect to linguistic innovation. In our discussion here we propose to be neither pro- nor anti-normative, but to insist that correctness in language is a linguistic problem, and that as such it is worthy of the attention of linguistic science. The anti-normative attitude is by no means original with American linguistics. In the nineteenth century linguists early began making the distinction now accepted between descriptive and prescriptive linguistics; Esaias Tegnér, the Swedish linguist, wrote (1874: 104) that the business of linguistics was not "to prescribe the laws of language, but to describe them" (*inte att skriva språkets lagar, utan att beskriva dem*). However, the line between these two activities is a thin one. And in our day of social science, the description of norms and values and the process by which they are arrived at is not regarded as an entirely unscientific procedure. Our problem today will be to distinguish these activities and see just what linguistic science and scientists can contribute to them. Even if this is not a pure science, it is unquestionably an application of linguistic technology which will classify as one branch of applied linguistics.

## 2. THE NATURE OF LANGUAGE PLANNING

2.1. Normative or prescriptive linguistics may be regarded as a kind of management or manipulation of language, which presupposes what I shall here call "language planning" (hereafter written LP). PLANNING is a human activity that arises from the

need to find a solution to a problem. It can be completely informal and *ad hoc*, but it can also be organized and deliberate. It may be undertaken by private individuals or it may be official. Social planning is an activity with a well-defined scope in our society, though various countries find it palatable to varying degrees in particular areas. If planning is well done, it will include such steps as extensive fact-finding, the consideration of alternative plans of action, the making of decisions, and the implementation of these in specified ways.

2.2. This suggested model is applicable to LP also. LP is called for wherever there are language problems. If a linguistic situation for any reason is felt to be unsatisfactory, there is room for a program of LP. In an earlier paper I defined LP as "the activity of preparing a normative orthography, grammar, and dictionary for the guidance of writers and speakers in a non-homogeneous speech community" (Haugen 1959:8). I would now prefer to regard this as one of the outcomes of LP, a part of the implementation of the decisions made by the language planners. The heart of LP is rather what I referred to as the "exercise of judgment in the form of choices among available linguistic forms". Even more concisely, I think we can define LP as THE EVALUATION OF LINGUISTIC CHANGE. This is the opinion also of the Indian scholar P. S. Ray, whose stimulating book on *Language Standardization* has been invaluable in preparing the following paper. He describes prescriptive linguistics as "the search for reasonableness in the discrimination of linguistic innovations" (Ray 1963: 18).

2.3. It is of course possible to deny all scientific value to the process of evaluation and choice, since linguistics proudly declares itself to be a descriptive science. Without getting entangled in the problem of free will and determinism, however, we can safely say that the question of choice in language is still completely open. The fact that each individual has to learn language anew, and never learns precisely the language of his teachers, and that people can and do change their language in the course of their lives, is sufficient to guarantee that there must be some area of choice. In so far as this is true, we can speak of LP as an attempt to influence these choices. Like any evaluation, it assumes that there are standards against which different linguistic innovations can be evaluated.

2.4. We must not in advance, however, assume that we know what these standards are. LP is not committed in advance either to PROMOTING or PREVENTING change. It is not committed to advocating either UNIFORMITY or DIVERSITY among different speakers or groups. It is not committed to either resisting or encouraging borrowing between languages: it may work either for PURIFICATION or HYBRIDIZATION. It may advocate either EXPANDING or RESTRICTING the resources of a language. It is not committed to EFFICIENCY at the expense of BEAUTY; it may work for ACCURACY as well as EXPRESSIVENESS. It is not even committed to the MAINTENANCE of the language for which it plans: it may work for a SHIFT to some other language.

2.5. In presenting the following systematic account of LP, we shall adopt a plan that is suggested by the general approach of DECISION THEORY. The study of decision making is one of the favorite pursuits of social theorists in our day, and there can be



no doubt that the general pattern of decision making holds for LP also. Without pretending to master the intricacies of this field of study, I would like to suggest that the data here presented fit well into a sequence which we may call a decision procedure. To take a definition at random: "Decision-making results in the selection from a socially defined, limited number of problematical, alternative projects (i.e. course of action) of one project to bring about the particular future state of affairs envisaged by the decision-makers" (Snyder 1958: 19).<sup>2</sup> We shall consider the kind of PROBLEMS that give rise to LP, the kind of DECISION-MAKERS that have been involved, the ALTERNATIVES that have been proposed and the limitations on these, the principles of EVALUATION that have been applied, and the IMPLEMENTATION by means of which policies have been enforced. In a short paper like the present one, we can only hope to sketch the outlines of what is obviously a very large subject.

2.6. Before considering the problems that give rise to LP, it will be important to establish the respective roles of SPEECH and WRITING. It will be quite impossible even to enter upon the subject if we maintain the usual position of linguists as expressed in Bloomfield's famous dictum (1933: 21) that writing is "merely a way of recording language by means of visible marks". The aggressively pejorative form of this statement is understandable in the light of Bloomfield's didactic purpose. No one can deny the overwhelming importance for linguistic science of the realization that writing is historically secondary to speech, as well as in the learning and the life of an individual. However, in the study of LP we shall have to reverse this relationship. We shall have to consider writing primary and speech secondary. This may be one reason for the comparative lack of interest among linguists in LP: to them it turns things upside down. It considers as primary what the linguist regards as secondary and assigns value to something which the linguist considers only a shadow of reality. The reason for the reversal is given by the function of writing as the medium of communication between speakers separated in time and space. Its permanence and its importance for the community permit and require a different kind of treatment from that which is accorded natural speech. Instead of remaining a mere record, it comes to embody a code of its own, which can influence the community speech.

2.7. The relation of a written language to the idiolectal codes of its users can be analyzed into a dual translation. If we start with any given idiolect, a linguist can apply his techniques and come up with an accurate and exhaustive record of the idiolect in the form of a standard linguistic description. We may call this the speaker's GRAPHOLECT, a precise record of his idiolect. Even in the most favorable case there are perceptible differences between them. The grapholect differs from the idiolect in being (a) edited, (b) analyzed, (c) delayed, and (d) stabilized. That it is EDITED means that it contains none of the innumerable and unpredictable erroneous utterances and false starts of the real speaker (Hockett 1958: 142). That it is ANALYZED means that such sequential units as the phonemes and the words are separated instead of

<sup>2</sup> I owe this reference to Charles Moskos, and others to Carl Hempl (both Fellows at the Center for Advanced Study in the Behavioral Sciences, 1963-64).

being fused: /wàynčə télmiy/ appears as five or six successive units in any orthography I could imagine of English: why didn't you tell me? That it is DELAYED means that it is learned as a second language and will therefore call for greater reflection and elaboration than the idiolect. That it is STABILIZED means that it has greater storage capacity and carrying power, and therefore encourages repetition and stability in its forms. In applying these principles to the idiolect, the linguist has translated the idiolect into a new medium, with consequent change of techniques and loss of information. The user of the grapholect must do the same in learning to read and write.

2.8. But the grapholect is not yet an orthography. A second translation must take place to adapt it to the needs of other idiolects, or in short, A COMPROMISE OF GRAPHOLECTS. As pointed out by Martin Joos (1960: 257) an ideal orthography should provide some degree of morphemic stability by being morphophonemic; it should permit alternate interpretation of the symbols so that different idiolects can read their own sounds into it; and it should be uniform so that diverse speech habits can be translated into it. But this means that a standard orthography is to some extent independent of the speech habits of its users; it becomes a language of its own, not just a reflection of speech. Its learners have a double learning problem: the gap between speech and writing purely as an encoding technique, and the gap between their idiolects and those reflected in the writing. This second gap is the one that can lead to actual linguistic change under the influence of writing, for writing can generate its own speech by being read aloud. By applying the translation rules he has learned, the reader can produce an idiolect different from his own to the extent that the grapholect is not identical with his. This effect is possible no matter how phonemically a language is spelled. In fact, a completely phonemic spelling will promote uniformity of pronunciation, so that spelling pronunciation is rather more likely in German than in English. Spelling pronunciations are unpopular among linguists, but a language like High German is inconceivable without them.<sup>3</sup>

2.9. The problem of STYLE in language is also important in a consideration of LP. While this, too, is closely associated with the distinction between speech and writing, it goes back well beyond the invention of writing. The distinction of styles according to situation has been documented by students of American Indian languages, who have used the term 'non-casual' speech for what I would prefer to call by its traditional term, 'formal' style.<sup>4</sup> We can identify certain genres of discourse which promote linguistic formality even in non-literate societies, such as LAWS, RITUALS, and EPICS. In these the language departs from that of everyday speech by being more dignified, distinct, and memorable. The reason is quite simply that these are of such importance to the life of the community that they must be memorized and passed on unaltered from generation to generation. They are entrusted to the LAWMAN, the PRIEST, and

<sup>3</sup> Pyles (1952: 241 ff.) writes: "Pronunciations based upon spelling, when these involve a change in the traditional pronunciation of a word in popular use, are a pretty sure indication not only of a spiritual arrogance which it is difficult to admire in the abstract but also of an ignorance of the relationship between writing and language."

<sup>4</sup> See Voegelin 1960, and other contributions to the same symposium.

the BARD, who thereby become at once the conservers of tradition and its potential innovators. The language which in this way comes down from the past is public and official, in a way that everyday talk is not. It is based on the situation of one speaking to many and for many; it is the voice of the group itself. If this is true in a non-literate society, it becomes increasingly so in a literate society, since writing enormously extends the size of memory storage as well as its accuracy. It would be most surprising if this did not have linguistic consequences in the form of a growing gap between casual and formal discourse, with the consequent high level of vocabulary retention over time (Zengel 1962).

2.10. Having now established the primacy of writing over speech for purposes of LP, we may make it into a general statement: LP refers primarily to the formal rather than the informal styles of language, especially in their written form. Any influence of the formal style on the informal is a secondary effect; the primary purpose is to influence the formal through changes in its written expression. This is the setting and background for all LP.

### 3. PROBLEMS OF LANGUAGE PLANNING

3.1. If we now consider the first aspect of LP, the PROBLEMS that give rise to it, they are all special cases of the problem of NON-COMMUNICATION. LP is in principle conceivable wherever there is failure of communication. But failure is not an either-or concept; it ranges along a scale from complete success to complete failure. If we take the extremes and the mean of this parameter, we may establish three kinds of communicatory situations: that of the face-to-face PRIMARY SPEECH-COMMUNITY where the only differences between speakers are idiosyncratic, or in linguistic terms, idiolectal; a SECONDARY SPEECH-COMMUNITY where there is partial understanding; and a TERTIARY SPEECH-COMMUNITY in which there is none, so that interpreters are required. Among political units we may instance Iceland as a primary speech-community, England as a secondary speech-community, and Switzerland as a tertiary speech-community. We cannot here be concerned with the overlappings of these concepts, but merely point out that the secondary speech-community is ripe for a NATIONAL language, the tertiary for an INTERNATIONAL, or AUXILIARY language. Both are instances of situations where LP can be called on for assistance and development. More generally, each of these needs a common code, an auxiliary language, which will enable those who wish to communicate with the members of other primary speech-communities than their own to do so (Gumperz 1962).

3.2. By emphasizing the importance of the spoken language and the individual informant, linguistics has singled out the primary speech-community as its special concern. In such a community LP is superfluous, since the immediate necessities of communication have provided the corrective to individual anarchy. Each learner is corrected on the spot by other learners and his older models, often by the bitter expedient of mockery, until he has learned to conform to the best of his ability. The

linguistic code is internalized by each member of the community. In this way the interference with communication which has been referred to as 'code noise' is reduced to a minimum. It seems to me that the model which is here suggested, on the basis of information theory, as most adequately describing the face-to-face situation of primary speech communication, can also be applied to the secondary and even tertiary speech communities. *Mutatis mutandis*, the larger speech community, in which communication must be mediated, is best served by a common code. This code lacks the immediacy of the speech situation; it must be more consciously shaped, and in so far as writing is involved, there is ample room for LP. The main point is that a written language lacks the self-correcting feature of speech; it needs a specialized class of guardians to provide this therapeutic effect.

3.3. In practice the problematical situations vary widely. At one end of the scale is the illiterate population without any written tradition or centralized government whatever, a situation that is rapidly growing rarer throughout the world. Where writing has been introduced, a number of distinct situations can be distinguished. Ferguson (1962) has proposed a scale for describing these, using WO-W2 as a scale for the extent of WRITING, and St0-St2 as a scale for the extent of STANDARDIZATION. W2 represents languages in which "original research in physical sciences is regularly published" and St2 languages which have "a single, widely accepted norm which is felt to be appropriate with only minor modifications or variations for all purposes for which the language is used". Even these "ideal states" are of various kinds and require subclassification; for example, Swedish, which he considers an example of St2, covers an area in which there are living dialects that are quite incomprehensible to each other and to speakers of the standard. For speakers in Dalecarlia there are many occasions when Swedish is not felt to be appropriate. Countries like the United States or Iceland, to take one large and one small example, are much closer to the "ideal state" of St2 (Benediktsson 1962). Whatever the situation with respect to writing or standardization, there are problems for the language planner.

3.4. If the language is unwritten, there is the problem of providing an ORTHOGRAPHY. If it has an orthography, it may be or have become INADEQUATE for the needs of its users; or there may be COMPETING ORTHOGRAPHIES. Even if the orthography is adequate or is so well established that there can be no reasonable hope of change, there may be VARIATIONS within the standard which are subject to evaluation. These may affect pronunciation, grammar, syntax, or lexicon. Opinions can differ on which if any is either desirable and therefore to be promoted, or undesirable and therefore to be discouraged. It appears that such judgments are made in all speech communities, from the simplest to the most complex. They are not even limited to literate communities. Bloomfield (1927) found that his Menomini informants in Wisconsin, much to his surprise, had strong opinions concerning the quality of language used by their fellow speakers. He concluded that "by a cumulation of obvious superiorities, both of character and standing, as well as of language, some persons are felt to be better models of conduct and speech than others".

## 4. THE ROLE OF THE LANGUAGE PLANNER

4.1. This suggestion leads us now to consider the PLANNER himself, as DECISION-MAKER. Or in Dwight MacDonald's loaded form (1962: 259): "What kind of authority, if any, should attempt to direct and control change?" Prior to the Renaissance the care of language was in the hands of the grammarians and rhetoricians. Quintilian in his *De Institutione Oratoria* (ca 95 A.D.) defined one of the functions of grammar as that of "forming right speech" (*recte loquendi scientiam*) (1875: 29). The grammars of Greek and Latin came, predictably, well after the classical periods of these languages, and were essentially CODIFICATIONS of an already accepted norm. Codification is usually assumed to be one of the marks of a standard language, but it makes a difference whether it is merely a linguist's description of an accepted norm, say in literature or polite speech, or is conceived of as fixing or even creating that norm. The term codification means simply the explicit statement of the code, in the form of an ORTHOGRAPHY, a GRAMMAR, and a LEXICON. But the attitude towards the codifier and his own conception of his role has changed sharply through time, along with the meaning of the word "code". To those who thought of language as having divine origin, the codifier was a pundit, dispensing God's truth to the people. Successively the code has been regarded as law and the codifier as lawgiver, as etiquette and the codifier as arbiter of fashion, as national symbol and the codifier as a national hero. To estheticians he has stood as a champion of the norms of beauty, to logicians as the upholder of rationality, to the philosopher as interpreter of the laws of thinking. Now that information theory has given us a new meaning of "code", we are prepared to think of a codifier as a linguistic technician. But as social scientists we must be prepared to recognize that all the meanings of code and all the roles of the codifier which I have suggested still exist and enter into the complex function of language planning in human society.

4.2. It is noteworthy how the first grammars and dictionaries of the modern languages coincided with the rise of their countries to wealth and power in the fifteenth and sixteenth centuries. A typical instance is the first Spanish grammar, Nebrija's *Grammatica de la Lengua Castellana* of 1492, which he dedicated to Queen Isabella and called "a compañero del imperio" (Daube 1940: 31). The first academy devoted to winnowing out the "impurities" of a language was part of the same movement, in this case the establishment of Florence and its Tuscan dialect as the model of Italian. This was the Accademia della Crusca of 1582, which was the model of Cardinal Richelieu's Académie Française of 1635. The canny cardinal dictated the statutes, no doubt as part of his policy of political centralization, asking its members "to labor with all the care and diligence possible to give exact rules to our language and to render it capable of treating the arts and sciences" (Robertson 1910: 13). The example was followed, among others, in Spain (1713), in Sweden (1739), and in Hungary (1830). The chief visible fruits of the academies were dictionaries, beginning with the first known monolingual dictionary, the *Vocabolario degli Accademici della Crusca* (1612).

There was great agitation in England on the part of many notable writers of the 17th and 18th centuries, such as Milton, Dryden, Defoe, and Swift, calling for an English academy (Flasdieck 1928). But the English resisted any French idea, especially one in which they could sniff the odor of authoritarianism, and in the long run they accepted instead the decrees of a private citizen, Samuel Johnson, whose dictionary (1755) became the first important arbiter of English. The United States declared its linguistic independence of England by substituting another private citizen, Noah Webster, for the English Johnson.<sup>5</sup>

4.3. The heaviest demand for codifiers arose, however, in the nineteenth and twentieth centuries, primarily as a consequence of the American and French Revolutions and the spread of literacy. Reaching the masses was a problem of teaching, and books were the instruments of instruction. The technological demands of the printing press required standardization. Some populations awoke to the fact that they were being pressured into a new language, and were in fact second-class citizens in their own country. Political upsets led to the rise of new nations, or the revival of old ones, and we see in country after country the establishment of new languages as the result of codifications by individuals, by government commissions, or by academies. Names like Korais in Greece, Aasen in Norway, Štur in Slovakia, Mistral in Provence, Dobrovsky in Bohemia, Aavik in Estonia, and Jablonskis in Lithuania come to mind. These men were part linguists, part patriots, and their work left something to be desired as pure linguistics. Yet some of them contributed strongly to the growth of the science; for example, Aasen was also the founder of Norwegian dialectology. In all European countries with a universal public school system the Ministries of Education exercised control over the orthography and grammar of their language. In Turkey the dictator Kemal Atatürk established a semi-official Turkish Linguistic Society in 1932, filled it with party members and school teachers, and gave them the job of planning the reformation of Turkish after he had officially abolished its Persian script and substituted Roman (Heyd 1954). Between the extremes of private initiative and dictatorship, there is a wide range of organizations which have undertaken LP on behalf of some linguistic form, whether churches, societies, or schools of literature and science.

## 5. ALTERNATIVES IN LANGUAGE PLANNING

5.1. Let us now consider some of the ALTERNATIVE COURSES OF ACTION that are open to a program of LP. We shall here confine our attention to the secondary speech community, and of these particularly to the nation, since as Ferguson justly observes (1962: 25), this linguistically neglected entity is after all the usual basis for "communication networks, educational systems, and language 'planning'". There are sub-

<sup>5</sup> On Webster's views see his *Errors of Grammars* (1798) (quoted in Leonard 1929: 223), where he objected to adopting "the practice of a few men in London". Cf. also his preface to *An American Dictionary* (1828), reprinted in Sledd & Ebbitt 1962: 32-39.

national groups like the Welsh and trans-national groups like the Jews who have language problems of the same order as nations, but lacking official organs of support must get along as best they can in their language planning efforts. Much of what is here said applies equally to them, and only space prevents me from discussing their special situations.

5.2. In considering courses of action we need first to ponder some of the GOALS of linguistic behavior. We have so far assumed that this is rapid and effortless communication. But the basic model of communication, as suggested by Bühler and elaborated by Jakobson (1960), makes it clear that communication is not limited to purely referential conveying of information. There is that expression of ego which Jakobson calls EMOTIVE and the appeal to the listener which he calls CONATIVE; beyond these are such minor functions as the PHATIC, the METALINGUAL, and the POETIC. In terms of the social situation there is here involved an intricate interplay between the speaker and his audience, who may be taken to represent the community. He is expressing himself, but only that can be expressed which his community is ready to accept. Language does not merely serve as a means of social coöperation, but also as a means of individual expression. The first leads to UNIFORMITY of code, the second to DIVERSITY. The actual result has to be some kind of balance between the two.

5.3. For this reason we cannot, as stated earlier, identify the goal of planning as necessarily an absolutely uniform code, either in time or space. This is one of the errors that some would-be planners make: they wish to fix a language for all time, or to impose a single standard on speakers with the most diverse dialects. But planning can either envisage the replacement of MANY BY ONE or of ONE BY MANY. There can be planning for diversity as well as for uniformity, for change as well as for stability. Havránek (1932, 1938), in defining the nature of a standard language, described it as having STABILITY with FLEXIBILITY (cf. Garvin 1959). This is nothing peculiar to a standard language; it is the definition of any language norm, even that of the primary speech-community. Stability is the diachronic correlate to uniformity, while flexibility is the diachronic correlate to diversity. While a living language norm is predominantly stable and uniform, it provides for its users a varying margin of elasticity and diversity. As Hoenigswald has pointed out (1960: 27 ff.), all language change can be defined as REPLACEMENT, which may be either a split or a merger or neither. Evaluation follows the same course: it may decide in favor of many rather than one or one rather than many.

## 6. LIMITATIONS ON LANGUAGE PLANNING

6.1. Before making any such decisions, however, it is necessary to establish the LIMITATIONS OF CHANGE. The planner enters the situation at a given point in time and space. His first task will be to identify the language in question. This may not be an easy thing to do. When the Norwegian language reformer Ivar Aasen set out to recover the Norwegian language from its eclipse in the Middle Ages, he had to identify

the language as a norm existing only in a variety of rural dialects, which most people assumed were degenerate varieties of Danish. Prior to the work of Štur in Slovakia, at about the same time, the dialects of that country had been identified as varieties of Czech, a language with a much longer written tradition. One way for the planner to identify a language is to establish its history. By a combination of internal reconstruction and comparative linguistics, these men created norms which took their places as apparently lineal descendants of older languages spoken or written in these areas. In this way linguistic geography and history combine to fix limits to the possible proposals that can be made.

6.2. There is a further limitation on the projections of the planner. This is constituted by the state of the spoken and written traditions which he proposes to reform. If the standard proposed is for a previously illiterate nation, he need consider only the speech norms of his community. If there is only one norm, his problem is largely the technical one of providing an orthography, as we have noted earlier. If there is more than one, he is faced with a set of choices which will call forth the whole panoply of problems and the need for the kind of evaluations which we will shortly discuss. If, in addition, there already is one or more orthographies in existence, his task is further complicated, unless he chooses to build on one of these and merely patch it up in some way or other. In any literate community with some tradition behind it there is a whole set of convictions and rationalizations concerning speech and writing, against which the planner may turn out to be powerless, unless he can turn them to account for his own purposes. However irrational they may seem, they generally turn out to bolster the status and convenience of those who have invested effort in learning the social traditions.

6.3. In view of the sharp limitations on the alternatives, one might despair of ever securing any change, if it were not that people and societies do change and their languages with them. In evaluating the possible solutions to the problems we have broached, there is need of a decision procedure which classifies the alternatives. The alternatives must be classifiable in terms of some objective CRITERION, e.g. as longer or shorter, as older or younger. These in turn must be associable with some PURPOSE of wider concern, such as communication, self-enhancement, or group stability.

## 7. CRITERIA FOR LANGUAGE PLANNING

7.1. Those who have written extensively on this topic have offered a variety of criteria of evaluation. Adolf Noreen (1892) rejected the classical view that the models are to be found in the past and the organismic view that language is an evolving organism beyond the control of man's reason. He proposed a set of criteria for "wrong" language based on what he considered a reasonable approach: that is wrong which is misunderstood, or not understood, or understood only with difficulty; also that which is hard to pronounce or to remember, or is longer or more complicated than necessary, or is novel without adding anything to the language.



7.2. Most of these criteria are aspects of what we may more concisely sum up as the criterion of *EFFICIENCY*. The earliest formulation of it which I happen to know was made even before Noreen, under the impact of modern science, by the Swedish linguist Esaias Tegnér, who wrote (1874: 104) that the best language was "that which, most easily uttered, is most easily understood" (cf. also Noreen 1892: 113, Jespersen 1925: 88). The important part of this dictum is that it recognized clearly that the interests of the speaker may not be identical with those of the hearer. In practice, communication is achieved by a precarious balance between the speaker's economy of utterance and the hearer's economy of perception. Writing lacks the immediate correction of an audience and therefore requires that the writer anticipate the reader's needs. It lacks a number of the devices of speech for securing clarity, such as intonations and gestures, and must make up for these by explicit signals. For the experienced reader some of this may even be supplied by unphonemic spellings, which are so unpopular among spelling reformers and children.

7.3. There is a clear conflict of interest here between the *haves* and the *have-nots*, in this case those who have learned and those who have not. A system that is simple for those who have learned it may be extremely difficult to learn. Chinese characters are said by those who know them to be more rapid to read than alphabetic representations of the same units; but the cost of learning is clearly very high. English spelling bears only an imperfect relationship to any kind of English pronunciation; but its very unphonemic quality gives it morphemic identity, and for other European learners a greater familiarity and ease of learning than if it were phonemic. Efficiency must therefore be interpreted in terms of the relative cost of *LEARNING* to that of *UNLEARNING*. Since those who would have to unlearn generally control the social apparatus, while those who would learn are school children or foreigners, it is obvious that we have here a very strong brake on any form of change. But the general principle is still valid: a form is *EFFICIENT* if it is *EASY TO LEARN* and *EASY TO USE*.

7.4. We must above all avoid such facile notions as that shorter words are necessarily more efficient than longer words, or that a grammar without case endings is more efficient than one with. As P. S. Ray reminds us (1963: 41), there is a real economy in having text-frequent words as short as possible, and in natural languages we find that the function words usually are. Rare words, on the other hand, which are likely to be missed by the hearer, are more efficiently coded as long words since this makes them more redundant. Such words are also more usefully complex and therefore grammatically transparent than the more frequently used words. A language like English which for Indo-European has a minimum of inflectional endings has on the one hand an extremely complex system of prepositions, on the other a rigid word order which requires its written style to be more explicit than other languages of the same family in the introduction of such empty fillers as "do" and "one" (e.g. "I do say", or "the young one").

7.5. It should be a sufficient warning of caution, in the establishment of the efficiency of one linguistic form as compared with another, to recall that during the

nineteenth century two diametrically opposite judgments were made of the change-over in English from a so-called synthetic to analytic structure. The Romantics called it *DEGENERATION*, while the Evolutionists called it *PROGRESS*. Today we call it merely *CHANGE*. We have learned that one cannot take isolated bits from a structure and judge its efficiency from these; one has to evaluate each item in terms of the whole structure, and we still have far to go before our opinions in this area can become much more than speculations.

7.6. Another criterion which is the special concern of LP, wherever it occurs, is one that I shall call *ADEQUACY*, a term I prefer to that of Ray, who calls it "linguistic rationality" (1963: 45 ff.). It includes also what Garvin (1959), following Havránek, calls "rising scale of intellectualization". Both of these writers were thinking primarily of the capacity of the language to meet the needs of its users as an instrument of referential meaning. This is one of the points at which LP is brought into play, for example, in creating terms to correspond to the needs of modern science. In the primary speech community devices are always available, either through borrowing or creation, to extend the lexicon as far as its speakers need for everyday purposes: we all know that Arabs are concerned about camels and Eskimos about snow and therefore have highly developed vocabularies in these areas. In the secondary speech community such needs are often stimulated by contact with other nations and translations from their languages.

7.7. But there is another kind of adequacy which is nourished within the nation. This is less likely to affect the purely rational area, but rather fall within the more intimate personal life of its users. A language is extended in adequacy not only by an extensive terminology of science and philosophy, but also by a well-developed terminology of emotional and poetic expressiveness. LP may call for the encouragement of words from rural dialects, in order that a more vivid and homely flavor be imparted to its writing. This has actually been the case in Scandinavia, where language planners in all the countries long have regarded a study of the dialects as a stimulus to enrichment of the standard languages. The rule for adequacy is that a form must convey the information its users wish to convey *WITH THE DESIRED DEGREE OF PRECISION*.

7.8. The third criterion is *ACCEPTABILITY*, by which I mean much the same as Ray when he sets up the criterion of "linguistic commonalty". This is the sociological component of evaluation. It corresponds to what previous students of the subject have called *USAGE*, as a standard of correctness. Jespersen (1925: 133) pointed out that there are three types of usage: the *INTELLIGIBLE*, which meets the barest minimum of communication; the *CORRECT*, which meets all the conventional requirements of the language norms; and the *GOOD*, which meets certain higher standards of either clarity or beauty, and therefore stirs the admiration of its audience. Actually, these differences refer to varying degrees of acceptability within the secondary speech-community. Since this community is neither homogeneous nor completely heterogeneous, it shows a complex pattern of disjunctions which build up usage differentials and consequent drifts in language. The terminology developed by Ray (1963: 61) for this situation is

useful: there is a sub-set of users called the "lead", who are regarded as imitation-worthy and therefore have "prestige". The other users may imitate their usage to the extent that they have "access" to it, which will produce the "spread" of their usage.

7.9. This corresponds to the observation made, for example, by Antoine Meillet (1928) concerning the standard languages of Europe: "They are languages created by and for an élite." The whole problem is an extremely intricate one, of course, since the presence of an élite, even for some centuries, does not in every case lead to the triumph of its language. Classical languages like Greek, Latin, and Arabic washed across the shores of the Mediterranean in successive waves, absorbing populations of other language groups. Turkish did much the same, but in the end retreated and was confined to a relatively small area, as was Greek (Brosnahan 1963). Even in ancient times some populations developed resistances to the spread of major languages. In modern times this resistance is bolstered by the growth of linguistic NATIONALISM. While Meillet deplored what he called the "linguistic balkanization" of Europe in the twentieth century, there are grounds for thinking that linguistic nationalism is one of the more attractive aspects of nationalism. Indeed, a revaluation of nationalism itself appears to be taking place among younger American scholars, who are beginning to realize that nationalism is a step in the direction of internationalism rather than the reverse (Geertz 1963). It means in the first instance an encouragement to the individual citizen to think not only of his personal or local concerns, but also of those which affect his larger community. Nationalism has welded together the people of many primary speech-communities into secondary speech-communities, and thereby counteracted the parochialism and narrowness of the former. This function of the national standard is one reason for its value as a symbol of the nation. It forms an instrument of communication, not only by rulers to subjects or citizens, but also among the citizens themselves. They become part of a larger network, even if they also yield some portion of their linguistic distinctiveness.

7.10. At the same time the standard forms a CHANNEL OF COMMUNICATION with the outer world, at least to the extent that translations from other languages are made into it. While its user may be excluded from direct participation in the life of other nations, he gains greater access to them than he otherwise would, simply by having a language which is capable of rendering the ideas of other, possibly more significant, nations. The national standard is thus under influence from the outside world, at times even threatened in its distinctiveness by the languages of that world. Like the nation, it wears two faces: opposing disunity within and absorption from without.

7.11. Whether the planner is setting up a national standard or attempting to modify it, he is faced with choices as to whose usage he is to follow in so far as he chooses to build on speech. In most European nations it is accepted that the "lead" is an élite which is marked either by wealth, power, birth, education, or all four together. In newer nations and more recent times, however, it has come to pass that this élite is either non-existent or is deliberately bypassed. Those who advocate Demotike in Greece, Gaelic in Ireland, or New Norwegian in Norway have set themselves against

the current élite and made themselves a counter-élite, whose usage is built either on rural dialects or on everyday urban speech. The problem is sometimes envisaged as one of NUMBERS (the most widespread usage) or QUALITY (the best and most genuine forms) or of SOCIAL JUSTICE (reducing the learning handicap of the poor). But it may also be a conflict of geographically separated dialects, each claiming its rights as a language. The rule of acceptability is that a form must be ADOPTED OR ADOPTABLE BY THE LEAD of whatever society or subsociety is involved.

7.12. The three rules of efficiency, adequacy, and acceptability may either diverge or converge in any concrete instance. They overlap to some extent and interact in various ways, and their relevance differs according to circumstances. But they should enable anyone who contemplates a program of LP to make certain decisions.

## 8. IMPLEMENTING LANGUAGE PLANNING

8.1. In all such programs there comes the time when the decisions are to be implemented. How exactly has this been done? The linguist with his grammar and lexicon may propose what he will, if the methods that could assure acceptance are missing. Research on this problem is essentially one of mass media and would perhaps be more appropriate for a student of advertising than of linguistics. In the end the decisions are made by the users of the language, the ultimate decision-makers.

8.2. PRIVATE INDIVIDUALS have no other weight than their personal or professional authority. Those who attack such authority are prone to regard it as "authoritarian", while those who favor it will prefer the word "authoritative". The economy of being able to appeal to authority for decisions on matters that in themselves are too trivial to compel attention is very considerable. Norm-deviant spellings draw attention away from the subject matter, but in certain kinds of literature (e.g. light verse) this may be precisely what is desired. Without a norm, such deviations are meaningless, becoming mere free variation. A Samuel Johnson or a French Academy are alike in establishing a norm which by virtue of being explicit gives meaning to linguistic behavior. Style is understandable only in terms of deviation from norms, and the rise of subnorms.

8.3. GOVERNMENTS have the advantage over private individuals of having control over the school system, through which it is possible to train or retrain a population in its writing habits. The success of such programs varies greatly, according to the extent of need which they fulfill in the population. Atatürk was successful in changing the script of his language, but his attempts to purify it of Arabic and Persian words met solid resistance and had to be soft-pedaled. Languages like Lithuanian (Hermann 1929) and Estonian (Ross 1938) have had great success in the coining of new words for intellectual phenomena, while similar attempts in modern German or Danish have fallen flat (Tauli 1948).

8.4. If we consider the implementation of LP as a problem in language learning, we see at once that it resolves itself into the usual dilemma of the language teacher.

Language teaching has basically only two kinds of instruments at its disposal: (a) models for imitation in the form of spoken or written texts, provided by INFORMANTS; and (b) a set of rules known collectively as grammar, provided by LINGUISTS. One can learn a language best from informants, who provide models of behavior; but for adults it is helpful to supplement this with explicit rules. The reverse is not true: one may learn ABOUT a language from rules, but without informants and their texts one is not likely to learn a language at all. The best planner is therefore the one who not only codifies but also produces text. For this reason authors are necessary in the case of a written language, just as speakers in the case of speech. The Norwegian Aasen did not only codify his language, but also wrote poetry and prose which established the possibility of writing it and inspired others to follow his example. Interested organizations can provide incentives and sanctions, from authors' subsidies and prizes to the elimination of potential linguistic rivals by official fiat. Prestige must somehow be established, and opportunity ("access") provided for those who wish to learn. The most important thing is to make sure that those who are to do the learning are motivated by some genuine advantage which will accrue to them in return for their effort. At the very least it must strengthen their self-respect, by making them better and more valuable members of the society in which they live.

8.5. The simplest STRATEGY is to introduce the reform in the schools and let it gradually grow up with the children; but this may only result in social confusion. If English spelling were to be reformed, I think we should pray that it be a sudden rather than a gradual change. But this would require the act of a dictator like Atatürk, or else total consent in advance, neither of which appears to be in the offing.

## 9. THE ROLE OF LINGUISTS

9.1. We are now ready to provide what answers we can to the question of the role that linguists can play in a program of LP. It must be granted that linguists are not necessarily equipped to pursue language planning. Their sensitivity to linguistic nuances may be dulled by excessive preoccupation with the mechanics of language. But even a Linnaeus could love flowers, and there is nothing to prevent a linguist from loving his language. Literary men rarely possess the technical knowledge of linguistic patterns to be able to say exactly what they are doing with language. One might as well ask a great dancer or violinist to explain his art. Ability to perform is not necessarily coupled with ability to analyze. If language planning, as suggested earlier, is "the evaluation of linguistic change", it is necessary to know just what change is and how it relates to the past and present structure of the language. The great task of nineteenth century linguistics was to make clear the fact of linguistic change: all languages change and in regular ways. The regularity proved to be so great that many linguists became determinists, who denied the possibility of influencing change. Since language planning involves the deliberate direction of linguistic change, it will be rejected by anyone who takes a consistently deterministic position. On the other hand,

the fact that other social institutions can be and have been changed by formal planning has given hope to those who find inadequacies in their language or its writing. The real problem comes in the evaluation itself: for the literary man's hunch, the linguistic scholar attempts to substitute explicit rules. Here twentieth century linguistics has supplied precious information on the nature of language as a functioning medium of human communication. Only a full understanding of this aspect can provide the knowledge necessary for a useful evaluation.

9.2. There is also the historical fact that linguists have participated in all successful programs of LP in the history of the world, either as technical consultants or as prime movers. The urgent need for such planning by linguists in the developing nations of Africa is strongly emphasized by the Conference Report of the Leverhulme Conference held at University College, Ibadan, Nigeria, in 1963: "... There is an urgent need [in Africa] for information on which practical decisions concerning language can be taken. ... If, in these circumstances, linguists maintain a critical, detached, 'ivory-tower' outlook on the language situation and the language problems, they will certainly be detached from any role in some of the greatest experiments in the twentieth century, and lose the opportunity of contributing to the general development of the continent, in those fields in which they can make a more useful contribution than any other group of workers" (Spencer 1963: 136).

9.3. What are some of the special types of information linguists can and must bring to the problem? I would say there are four ways in which they can help; there may be others I have overlooked. The linguist can contribute as 1) historian, 2) descriptivist, 3) theoretician, and 4) teacher.

As HISTORIAN, the linguist can establish the history of the language. His studies make it possible to trace the continuity in speaking or writing by a group as far back as records or reconstruction can go. He can distinguish native from borrowed elements in the language, thereby providing a basis for purification or hybridization, as the case may be. For whatever value is placed on tradition, he can provide it. Some periods, such as the Romantic era, feel their self-respect enhanced by an honorable ancestry; others, perhaps more realistic, are less excited by it.

As DESCRIPTIVIST, the linguist can provide accurate descriptions of actual present-day practice, both in speech and writing, of the community involved. As a dialect geographer he can gather information about variety and unity in the popular speech; as a modern student of social dialect he can establish the prestige value of various forms. He can observe and classify the phenomena associated with standard speech and writing, so that the actual practice of the community may be known. He can embody the results of his research in an orthography, a grammar, and a dictionary.

As THEORETICIAN, he can establish some of the guide lines for an understanding of language in general. More specifically, through his knowledge of linguistic techniques of analysis and the universals of language typology, he can make statements about the design of languages. His understanding of the relation of speech to writing enables him to say what one can expect from the manipulation of either one. He realizes the

importance of unitary structure for efficient communication at the same time as he allows for individual deviation. He has studied previous programs of language planning and observed the extent of their success, so that he can distinguish the "natural" from the "artful" changes.

In so far as he is also a TEACHER, the linguist keeps an eye open for problems that are likely to be involved in the training or retraining of language users. His experience and training should enable him to judge whether a particular project is pedagogically feasible and desirable. He can estimate the amount of effort currently expended on learning, and can feed back information to himself and other planners on the attitudes of his pupils toward the project involved.

9.4. Lest it be thought that we are proposing either that linguistics be identified with LP, or that LP needs only employ linguists in its project, I hasten to add in conclusion that linguistics is NECESSARY but not SUFFICIENT. In so far as LP is a kind of language politics, it needs the insights of POLITICAL SCIENCE concerning the art of what is possible and the ways of winning the consent of the governed. Since it is deeply involved in man's behavior as a social animal, it needs the support of an intelligently conceived ANTHROPOLOGICAL and SOCIOLOGICAL theory of linguistic behavior. For whatever contribution PSYCHOLOGY can make concerning learning behavior and modes of perception, there is more than enough room in LP. Nor can we overlook the contributions of the ESTHETICIANS and the PHILOSOPHERS. Even though a Dwight MacDonald may be badly informed about linguistics, he voices a point of view which cannot be disregarded if we would not wish to be considered barbarians. Even if linguistics in its pure or MICROLINGUISTIC sense can narrow its view to the microcosm of language, an applied linguistics which claims to be included in MACROLINGUISTICS cannot overlook the macrocosm of society in which we actually live, speak, and write.

#### DISCUSSION

BIRNBAUM: I was interested in your reference to L'udovít Štúr and his achievements with Slovak, and I think there are very interesting and important parallels with the creation of Landsmål by Ivar Aasen. On the other hand, there's a slight difference, since in the Slovak case we have to do with the attempt to create a literary standard language on the basis of a number of dialects which up to that time were considered only varieties of Czech. A similar situation is actually prevailing even today in Poland, but leading to almost the reverse result. We have the case of Kashubian; attempts were made to establish Kashubian as a literary language separate from Polish, but today I would think the tendency is rather not to allow Kashubian to emerge as a literary language, but to consider it as a more remote type of Polish dialect.

HAUGEN: As far as Štúr is concerned, there's one especially interesting parallel, namely that before Ivar Aasen's work, it was commonly assumed that the Norwegian dialects were degenerate forms of the Danish language. They were degenerate because

they were spoken by peasants, and they were a form of Danish because Danish was the official language. This was of course a common European assumption, and still is in many circles. The very term 'dialect' is created out of that concept. This is one reason I don't like it as a scientific term at all, because it normally has this pejorative sense.

BIRNBAUM: In the context of Slavic parallels we should emphasize the parallelism with Serbo-Croatian, where the achievements of Vuk Karadžić to a very high degree parallel the situation with Ivar Aasen. Aasen tried to create a literary language on the basis of dialects, even though the official language in the area was a foreign language — though it was a closely related language, namely Danish. Something very similar happened in the Yugoslav territory, where Karadžić actually tried to create a new language, primarily on the basis of one dialect. The previous literary standard, if any such did exist at that time, was something that didn't have the same roots.

HAUGEN: I am quite aware of Karadžić's work, although I will confess it is only recently I have been learning about it. But Professor Gallis at the University of Oslo has written some excellent material on the Serbo-Croatian situation, including an article called "Vuk Karadžić, the Yugoslav Ivar Aasen" (1950).

P. IVIĆ: In connection with Aasen and Karadžić, there is really a strong parallelism between the two cases. I remember that as a schoolboy, in high school, I read an article in a Yugoslav publication, with the title "Ivar Aasen, the Norwegian Vuk Karadžić".

In connection with Karadžić, I wanted to discuss the principles of orthography given by Martin Joos (1960). None of them seems to me universal. All of them are necessary only for languages with certain types of structural or sociolinguistic complexities. There are morphophonemic complexities in languages like English, making necessary a morphophonemic orthography. But without alternations which can deform the shape of morphemes, you don't need morphophonemic orthography, so that phonemic orthography suffices; and this is exactly what Karadžić introduced in Serbia. As to the principle that an orthography should permit alternate interpretation of the symbols, so that people with different idiolects can read their own sounds into it — this is connected with another complexity, with the co-existence of two or more acceptable pronunciations of the standard language. If this does not exist, this condition too is unnecessary. The third condition is that the orthography should be uniform so that diverse speech habits can be translated into it. This again is connected with a type of diversification which is not necessarily present everywhere. Thus it seems to me that conditions for an ideal orthography vary greatly from language to language. In the case of a language like Serbo-Croatian, the ideal solution was that of Karadžić, simply a phonemic orthography.

HAUGEN: I think that your point is very well taken, because clearly the difference in structure between different languages will be a determining factor. Finnish, for example, seems to have no problem with a phonemic orthography because each morpheme is just there. Although there are complicated morphophonemic rules, they are not reflected. On the other hand, a simple example in Norwegian is the word



*dag* /dag/ "day" and its genitive *dags* /daks/. It is perfectly possible to argue that this should be written *daks*, because this is what people say, and since /k/ and /g/ are in opposition within the language. But writing *g* in the genitive keeps the forms of the word together. This shouldn't be carried too far, for it does create problems. There are other words, like the word for "scissors", which is written *saks* and rhymes with *dags*. For the children who write it, this is a complication. But in general there are many advantages in writing morphophonemically.

BIRNBAUM: Do you have any conclusions as to the desirability of this merging of Nynorsk and Bokmål into one common language in Norway? Are your feelings that this is something worthwhile supporting, or something rather detrimental?

HAUGEN: It would require a whole new lecture and paper to justify any opinion I could give on it. Personally all I can say is, I don't like it. But this doesn't mean anything at all, because whether I like it or not, some such form of fusion is going to take place. How fast it takes place is another problem. It may not take place for a hundred or two hundred years. But it may also take place much more rapidly, depending on the possibility of indoctrinating the young people into the concept of a common language. But it is a very difficult process, because all the traditionalists are against it — and as I indicated here, the traditionalists are always in a majority in any social group.

BIRNBAUM: I understand that today the people who take the students' exam have to write two compositions, one in Nynorsk, one in Bokmål.

HAUGEN: Yes, this is a matter of national tradition, insisting that everyone must at least know, passively, both languages, even if he doesn't actively use either one. In the long run, this can't help but result in an informal fusion, a kind of pidginization whereby, on the one hand, the standards of the educated classes are broken down because their children no longer are forced to submit to them, and on the other hand the education of the country people comes to a point where they will be using a standard form. So they will meet somewhere, but I would not be so rash as to predict when, where, or how.

GUMPERZ: I'm interested in the role of linguists as language planners, and this morning we have suggested that possibly our own cultural heritage has something to do with the way we do linguistics. I think a very good example of this is found in modern Asian societies. Since Pāṇini's time, Indian linguists have become more and more conservative. Their main concern has been the preservation of pre-existing grammatical conventions and literary tradition rather than the creation of new styles. It seems to me that this conservative role of Indian linguists creates some difficulties in the context of Indian modernization. At the present time India faces the problem of converting Hindi into a standard language, capable of serving as a communication medium in a modern nation-state. If this is to be accomplished, a new style will have to be created which, while uniform in spelling and grammar, is at the same time intelligible to a maximum number of people. When linguists have been called in to aid in this process, they seem sometimes more concerned with reviving Sanskrit forms than

with facilitating communication. It is interesting to see the changes they advocate and to observe the effect that these changes have had. In one group of commonly accepted Sanskrit borrowings, such as *raaj* "government", they have advocated the addition of a nominalizing suffix -y, thus reviving a final consonant cluster which no longer exists in modern Hindi and creating literary *raajy*, with the same meaning as *raaj*. Since uneducated speakers of Hindi have difficulty in learning the new cluster, innovations of this type have the effect of increasing the language distance between spoken and literary Hindi. This type of change cannot be justified on the grounds of communicative efficiency. Therefore, we might wonder whether linguists advocating this type of change are not confusing their role as language planners with their well-established role as preservers of the older literary traditions.

HAUGEN: The matter implies a value judgment, obviously, and that puts us right in the midst of a difficult problem. Something like what you described happened in France, in the Renaissance, when the natural French was interlarded with hundreds of loans from Latin, which of course was the language of law and of government, and was therefore thought of as superior to the broken-down forms of the Vulgar Latin, which had developed into French. And so we get these contrasts within French which are reflected in English borrowings, e.g. "king" versus "royal" and "regal". This situation in present-day Hindi is very similar.

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## DIALECT DIFFERENCES AND SOCIAL DIFFERENCES IN AN URBAN SOCIETY

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When kings have become philosophers and philosophers have become kings, when the average American citizen becomes less intensely competitive with his neighbors and more willing to give each one a change to do to his fullest capacity the work for which he has the greatest aptitude and interest, then — as class markers lose significance — it will hardly be necessary to talk about social dialects. But until that time, while a person's social standing will be assessed in terms of the way in which his use of English measures up to what the dominant culture consider the marks of educated speech, it will be important to understand the linguistic indicators of social difference in any given community. Everyone knows from his own experience that such indicators exist, though he may misjudge the actual significance of a particular item. As an example of the kind of mistake one can make, until I was nearly thirty I could not imagine that a truly educated and cultivated person would fail to distinguish between such pairs as *horse* and *hoarse* or *dew* and *do*, or such triads as *merry*, *marry* and *Mary*.<sup>1</sup>

But in compensation for such limits, I learned intuitively that in my native community (Greenville, South Carolina) one's vowels were clear markers of one's social standing. This experience prepared me for what I encountered in teaching in a wide spectrum of other communities — Charleston, South Carolina; Lafayette, Louisiana; Cleveland and Chicago — where complex social dialect situations could be readily observed.<sup>2</sup>

The intuitive and informal observations in these situations have been reinforced by some three thousand hours of field interviewing for the Linguistic Atlas project, and by the more intensive studies that younger investigators have undertaken — some of them, I am proud to state, students of mine. This paper presents a synthesis

<sup>1</sup> Such distinctions are characteristic of the Southern and South Midland dialect areas of the Atlantic Seaboard (Kurath and McDavid, 1961). A summary of features of some of the principal areas of the Atlantic Seaboard was prepared by the late E. Bagby Atwood, from materials in the Atlas archives; it has been freely adapted as a general reference, as in Chapter 9 of Francis, 1958.

<sup>2</sup> Phonetic summaries of cultivated Charleston and Greenville speech appear in Kurath and McDavid, 1961. Lafayette, as a bilingual community (English-Acadian French) has been studied by Wallace A. Lambert, of the psychology department of McGill University; Chicago has been studied by Pederson, 1964, and Cleveland by Drake, 1961. A study of the effects of urbanization on the speech of Akron, Ohio, by Gerald Udell, will be completed in the summer of 1965.

of some of the evidence gathered in these investigations; if any specific remark impinges on a sensitive corn, as that of a metropolitan school superintendent, the corn-bearer should not blame the evidence but the society in which the evidence was gathered.

A dialect, in the sense in which American scholars use it, is simply an habitual variety of a language, set off from other such varieties by a complex of features of pronunciation (/drin/ vs. /dren/ "drain"), grammar (*I dove* vs. *I dived*) or vocabulary (*doughnut* vs. *fried cake*). Dialects arise through regional or social barriers in the communications system: the stronger the barrier, the sharper the dialect differences. Most often we think of a dialect as the way some stranger talks; we generally assume that we speak "normal English" — or French or Russian or Burmese or Ojibwa, as the case may be.

The most obvious dialects, to most of us, are the regional varieties — the Eastern New England type, of the late President Kennedy; the Southwestern variety, of President Johnson; or the Charleston variety, which everyone in the Up-Country of South Carolina used to mock in something like the following:

[wi'l hav əleːt deːt əteːt and goːt əut tənəit ɒn ðə boːt ənd rəɪd baɪ ðə batrɪ ənd θroːt  
brɪkbatz ət ðə batlʃɪps]

"We'll have a late date at eight and go out tonight on the boat, and ride by the Battery and throw brickbats at the battleships."

Other regional varieties may be less conspicuous than these, but we generally do fairly well in sorting out the stranger from the person who grew up in our home town.

In addition to regional dialects, however, we have social dialects. A social dialect, as I define it, is an habitual sub-variety of the speech of a given community, restricted by the operation of social forces to representatives of a particular ethnic, religious, economic or educational group. By and large, the more that any one sub-variety is esteemed above all others in a given community, the sharper will be the distinction between it and its less-favored competitors. No community is without social dialects; but in general, the fewer the locally sanctioned class barriers, the more difficult to find the true class markers, in speech as in anything else. Since it is impossible to give in detail all kinds of social dialect situations, my discussion here is confined to two examples — Greenville and Chicago. Both illustrate the traditional pattern of urban growth from in-migration, though the scales and the details are different. For Greenville I draw on years of intuitive observations as a child and adolescent (observations the more likely to be objective since as a child I felt no identity with any local class or clique or cult), followed by field investigations for the Atlas. For Chicago I rely on conscious observations, especially on the dissertation of Lee Pederson.<sup>3</sup>

<sup>3</sup> In addition to his dissertation, Pederson has been actively involved in the Chicago social dialects project, 1963-5, and has collected a massive quantity of lexical evidence from the metropolitan area.

## GREENVILLE: A MICROCOSM

Greenville, like most of the Upland South, was originally settled by Ulster Scots and Pennsylvania Germans, who came southward along the Shenandoah Valley and the eastern slopes of the Blue Ridge. A few of the most adventurous drifted northwest with Daniel Boone and his associates, to colonize the Ohio Valley; another group, fiercely individualistic, infiltrated the Appalachians, where they made some of the best whiskey in the world (illegal, of course) until the industrialization of moonshining during the unlamented Noble Experiment. Most, however, settled down to family farming in the Piedmont. The town of Greenville, in the geographical center of the county, developed out of a village that grew up around a grist mill established by a local trader during the American Revolution. Here the county seat was located, with the establishment of counties in the 1790's; and a small professional élite, partly from the cultural focus of Charleston, partly from the other focus of Eastern Virginia, founded law firms, banks and other businesses. Early in the Nineteenth Century the village became a summer resort for rice planters and their families during the malaria season (roughly May 1-November 1); a few of these summer people became permanent residents and members of the local élite. Others from the Charleston area, including representatives of humbler families, refugeeed in Greenville during the Civil War. From the house servants of these Charleston-oriented families are descended many of the present-day leaders in the local Negro community.

With the spread of cotton culture following Eli Whitney's invention of the gin, the plantation system — and its concomitant, Negro slavery — spread to the southern half of the county, and the Negro population increased by 1860 to about  $\frac{1}{3}$  of the total. But the plantation interests never dominated the county. In particular, the mountainous northern half was unsuited to plantation agriculture, and the mountaineers were particularly resentful of Negro slave competition. The county was a stronghold of Unionist sympathy before and during and after the Civil War; contrary to the official myth, the percentage of desertions from the Confederate Army was high, and there were many echoes of the popular designation of the Lost Cause as "a rich man's war and a poor man's fight". A Greenvillian, Benjamin Perry, was governor of South Carolina during the short-lived period of reconciliation before Congress established the Reconstruction government. With emancipation, most of the plantation slaves became tenant farmers, and many Negroes continue in that status today, despite the drift from farm to city and thence to the North.

During the period of industrialization that set in toward the end of the Nineteenth Century, the county in general and the city of Greenville in particular became one of the centers of the Southern textile industry. The mills maintained the traditional segregated employment patterns of Southern industry established by William Gregg, at Graniteville, S.C., in the 1830's. Gregg had looked to the textile industry as the salvation of the poor white farmers crowded off the land by the spread of the plantation system, and as a refuge where they would be protected against the unfair competition of slave

labor. The continuing pattern has been for new mills to be staffed by displaced or unsuccessful farmers from the mountains and other unproductive areas, though by now textile employment has become a hereditary way of life for the fourth generation, who may move from mill to mill over a hundred-mile span, but who remain tied to the industry, basically one for low-skilled and low-paid labor, susceptible to long periods of depression and unemployment and bitter competition from the newer mills in the Deep South. Except for menial tasks, Southern cotton mills hire few or no Negroes, and the traditional threat of Negro employment is cannily exploited by mill management whenever the unions launch organizing drives.<sup>4</sup> As a result, the cotton mills around Greenville, as elsewhere in the South, remain essentially un-unionized. Until recently, most mill operatives have lived in company villages with company stores furnishing long-term credit on terms sometimes little short of peonage. These villages, like the mills, were set up outside the city limits to avoid city taxes. However, since World War II the company villages have begun to disappear, as the mills have sold off the mill-village houses to operatives and others.

The mill schools were rather poor at first, thanks to a district school system that till the 1920's received little help from the state.<sup>5</sup> About 1920, when the population of the mill belt had reached some twenty thousand, little less than that of the city proper, the management of the various mills joined to support a united and segregated school district, largely restricted to the children of mill operatives. This school system was independent of that of the city proper, which at that time provided a competent traditional education for local whites and a separate and inferior one for Negroes. So strong were the economic and social barriers setting off the mill district from the town that it used to be said, not altogether in jest, that Greenville was a community of three races — whites, Negroes and cotton-mill workers. The mill hands and the city people were as mutually distrustful as either group of whites and the Negroes — and the former distrust was repeatedly exploited by low-grade demagogues. Within the Negro community, of course, there were also competing groups; but of those the local whites were largely ignorant.

Negro speech, like educated white speech of the town, generally had loss of constriction of postvocalic /-r/; rural white, cotton mill and mountain speech generally retained constriction. Educated speech had [raɪt] but [ra'd]; uneducated speech normally had [ra't]. Thus *nice white rice* became a social shibboleth; for Negro, cotton mill, mountain and rural uneducated white speech most commonly had [na's hwa't ra's], while a few speakers with Charleston or eastern Virginia connections (or

<sup>4</sup> This was particularly evident during the fiasco "Operation Dixie", a widely publicized but poorly planned drive of the Textile Workers Union, CIO, following World War II. For evidence I am indebted to Earl Taylor, then of the Greenville TWU office.

<sup>5</sup> In this connection it must be recalled that there was no constitutional commitment to public education in South Carolina before the Reconstruction Constitution of 1868. With public education a much more recent commitment than, say, in the old Northwest Territory, it is not surprising that some spokesmen for the alleged "Southern Way of Life" have talked casually about abandoning the public school system, as an alternative to integration.

pretensions) had [nəis hwæt rəis]. A very few Charleston- and Virginia-oriented speakers (mostly women) affected the so-called "broad *a*" in such words as *half past* and *dance*. This pronunciation had no prestige; in fact, it was often an excuse for ribald humor. The Charleston intonation and vowel qualities would be tolerated in elderly distinguished citizens, but cruelly ridiculed in the young. Folk verb forms were common on all levels of speech in all styles, except in the most formal situations for the most cultivated. An educated speaker who would not use *ain't* in familiar conversation with his social equals was looked on with suspicion, as if attempting to cover up an unsavory past. Local and regional lexical items were used in everyday speech as a matter of course; a child might read about the *chipmunk* Nurse Jane in the Uncle Wiggly stories without identifying it with the *ground squirrel* in the city park.

#### CHICAGO: THE MACROCOSM

From this microcosm it seems a far cry to the macrocosm that is Chicago. Yet here too we may trace the chain of influence from the historical background to the sources of local speech patterns and the relationships of those speech patterns to the social order.

Northern Illinois — like northern Indiana, southern Michigan and southeastern Wisconsin — was first settled from the Inland Northern dialect region: western New England, by way of Upstate New York. In many of the small towns in Chicago's exurbia, the older families still show distinctly New England speech-traits, such as the centralized diphthongs [əʊ] and [əɪ] in *down* and *ride*, or /ʊ/ in *spoon* and *soon*. But the city of Chicago developed a more polyglot tradition from the beginning. The city was established at the time when the Erie Canal made it easy for the economic and political refugees from western Europe to reach the American heartland. The Irish brought reliable labor for the new railroads and a continuing tradition of lively politics; the Germans contributed their interest in beer, education, art, music and finance. Almost immediately Chicago also became a magnet for the younger sons of the agricultural settlements in southern Illinois and southern Indiana — Midlanders, whose speech patterns derived from western Pennsylvania. Scandinavians followed Germans and Irish; toward the end of the Nineteenth Century the population of the metropolitan area was swelled by mass peasant immigration from Southern and Eastern Europe — the strong backs and putatively weak brains on which Chicago's mighty steel industry was built. When this immigration tailed off during World War I, a new supply of basic labor was sought in the Southern Negro. Negro immigration has increased until Chicago is possibly the largest Negro city in the world. More recently the Negroes have been joined by Latin Americans (Mexicans, Cubans, Puerto Ricans), and last by rural whites from the Southern Appalachians. In response partly to the pressure of the increasing non-white population, partly to easy credit and slick promotion, Chicago whites like those in other cities have spread into the



suburbs, many of which are at least informally restricted to a single economic and social (sometimes even an ethnic or religious) group.<sup>6</sup>

In Chicago as in most large cities, the development of social dialects has been a by-product of what might be called differential acculturation: differences in the facility and speed with which representatives of various social groups develop the ability to live alongside each other as individuals, without stereotyped group identification. In favor of the trend is the traditional American principle of individual dignity, and the belief that each man should be allowed to improve his lot as far as his ability and his luck permit. Against it is the tendency of people to flock together according to their nature and common ties — whether Filipinos, Orthodox Jews, Irishmen, hipsters or college professors — a tendency abetted by those with a stake in keeping the flock from scattering and by the tendency of each group to reject the conspicuous outsider. In the early Nineteenth Century, the Pennsylvanians and downstaters, with a few generations of Americanizing under their belts, soon mingled freely with all but the wealthiest and most genealogically conscious Northerners. Acculturation was more difficult for the “clannish” Irish, Germans and Scandinavians.<sup>7</sup> The Irish were usually Roman Catholics; the Scandinavians spoke a foreign language; many Germans suffered from both handicaps, and all three groups had broken with their native cultures only recently and maintained many of their native customs. Nevertheless, all three of these groups had enough in common with the “Older American Stock” — all coming from northwestern Europe — to make some sort of symbiotic assimilation easy, though all of these older immigrant groups were to suffer during the xenophobic hysteria of 1917-19 and after.<sup>8</sup> In general they managed to participate freely in the community, while retaining their cultural societies, newspapers and even foreign-language schools.

The later immigrants from Southern and Eastern Europe suffered, in general, from the twin disabilities of foreign language and Roman Catholicism. Moreover, they were largely peasants and illiterate, without the strong sense of their cultural tradition that the Germans and Scandinavians had brought.<sup>9</sup> All these groups found themselves at the focus of a complicated polyhedron of forces. In an effort to help their acclimatization — and no doubt to avoid the erosion of traditional ecclesiastical allegiance — the Roman hierarchy fostered the “ethnic parish”, designed specifically for a single nationality or linguistic group. Whether or not this institution served its immediate

<sup>6</sup> For the socio-economic ranking of Chicago suburbs, see Pederson, 1964.

<sup>7</sup> It is obvious that only outsiders are clannish; in-groups are merely closely knit.

<sup>8</sup> Since the Germans were the largest functioning foreign-language group, and since their position in the cultural life of the community was so high, it is almost impossible to calculate to what extent the teaching of languages and literature, and the position of the scholar, suffered as a result of these witch-hunts. The abolition of Germanic studies at the University of Texas, as a gesture of patriotism, was only one of many such acts.

<sup>9</sup> Paradoxically, one of the most successful adaptations of native cultural traditions to the opportunities of the American setting — that of the South Italians of Chicago to the public demands during Prohibition — tended to stigmatize the whole group, whether or not they actively participated in the Syndicate's version of venture capitalism.

purpose, it had the side effects of further identifying foreignness and Roman Catholicism, of separating the new groups from the American Protestants, from the "native Catholics" (chiefly of Irish and German descent), and from each other, and of fostering ethnic blocs in local politics.<sup>10</sup> The blocs persist; but the common tendency of Chicagoans (as of Clevelanders) of Southern and Eastern European descent is to abandon their ancestral languages and turn their backs on their ancestral cultures, even in the first American-born generation. The notable exceptions are the Jews, with the attachment to the synagogue, to the synagogue-centered subculture, and to the family as a religious and culturally focused institution. But it is possible for an individual from any of these new immigrant groups to give up as much of his ethnic identity as he may wish, and to mingle relatively unnoticed in apartment building or housing development alongside members of the earlier established groups.

In contrast to both of these groups of European immigrants, the American Negro in Chicago is a native speaker of American English, normally of at least five generations of residence in North America (cf. McDavid, 1951); little survives of his ancestral African culture, though undoubtedly more than American Caucasoids are generally willing to admit. Early Negro settlers in Chicago were able to settle as individuals — whether freemen or manumitted or fugitive before the Civil War, or emancipated migrants afterward; furthermore, a large number of the earliest Negro immigrants were skilled craftsmen, who might expect to find a place in an expanding economy, and with some education to smooth off the rough corners of their dialects. However, even as an individual settler the Negro was more easily identified than any of the whites who had preceded him; and many Negroes exhibited traumata from slavery and mass discrimination. With the mass migrations of Negroes, other forces began to operate: the arrivals from 1915 on were largely a black peasantry, somewhat exposed to urban or small-town life but almost never actively participating in the dominant culture. Their own American cultural traditions — gastronomic, ecclesiastical and everything between — often diverged sharply from those of middle-class Chicago. Their speech, though American English, was likewise sharply different from that of their new neighbors. Even an educated Mississippian has a system of vowels strikingly different from the Chicago pattern. An uneducated Mississippi Negro would have had his poor sample of learning in the least favored part of the Southern tradition of separate and unequal schools; his grammar would differ more sharply from the grammar of educated speech in his region far more than would the grammar of any Northern non-standard dialect differ from the local white standard. Furthermore, the easy identification of the Negro immigrant would provoke open or tacit

<sup>10</sup> In the manuscript version of his *Language Loyalties in the United States*, Joshua Fishman repeatedly attacks the heavily Irish American hierarchy of the Roman church for discouraging the ethnic parish in the past generation. In contrast, the late Msgr. John L. O'Brien, director of parochial education in the Charleston diocese, was outspoken in his belief (based on his home community in Pennsylvania) that the ethnic parish tended to prevent the development of a truly Catholic church in the United States. The Irish, after all, had found their ethnicity one of their greatest obstacles to full participation in American society.

pressure to reinforce the tendency of living with one's kind — a situation which, for the Chicago Negro, is likely to strengthen the linguistic and cultural features alien to the dominant local dialect pattern. Finally, the displacement of unskilled labor by automation has injured the Negroes — less educated and less skilled, on the whole — more than it has other groups. The specter of a permanently unemployable Negro proletariat has begun to haunt political leaders in Chicago as in other Northern cities, with inferior educational achievement and inferior employment opportunities reinforcing each other, and in the process strengthening the linguistic differences between whites and Negroes.

Of the Latin American, it can be said that he adds a language barrier to the problem of physical identification which he often shares with the Negro. Of the displaced Southern mountaineer, it has often been observed that he is even less acculturated to urban living than the Negro or the Latin; however, his physical traits will make it easy for his children to blend into the urban landscape, if they can only survive.

What then are the effects of this linguistic melting pot on the speech of metropolitan Chicago? And — since I wear the hat of English teacher as well as that of social dialectologist — what are the implications for the schools?

First, the speech of the city proper has apparently become differentiated from that of the surrounding area, as the result of four generations of mingling of Inland Northern, Midland, and Irish, and the gradual assimilation of the descendants of continental European immigrants. The outer suburbs call the city /šikágo/, butcher /hagz/, and suffer from spring /fag/; to most of its inhabitants the city is /sikógo/, quondam /həg/ butcher to the world, beset by Sandburg's cat-footed /fəg/. To the city-bred, *prairie* and *gangway* and *clout* have connotations quite different from those they bear in the hinterlands.<sup>11</sup> Little if anything survives in the city of such Inland Northern speech forms as [əi] and [əu] in *high* and *how*, [u] in *soon* and *spoon*, or /éjə/ as an oral gesture of assent. Even the second generation of Irish, lace-curtained or otherwise, have largely lost their brogue; such pronunciations as /ohérə/ for *O'Hare* Field seem to be socially rather than ethnically identifiable.<sup>12</sup>

Among the older generation with foreign-language backgrounds, one finds sporadic traces of old-country tongues, such as lack of certain consonant distinctions (e.g., /t/ and /θ/, /d/ and /ð/) that are regular in standard English. Among the younger generation of educated speakers some of the Jewish informants stand out, not only for the traditional American Jewish vocabulary, from *bar mitzvah* and *blintz* to *tsorris* and *yentz*, but for the dentalization or affrication of /t, d, n, s, z, r, l/. The former features have spread to other local groups, but the latter has not. The so-called Scandinavian intonation of English is rarely encountered, even among in-

<sup>11</sup> Evidence on these features is basically that of Pederson's dissertation and his unpublished lexical research.

<sup>12</sup> The pronunciation /ohérə/, definitely substandard in Chicago, is however the normal one for Mayor Richard J. Daley, probably the most powerful municipal politician in the United States. To urban Chicagoans a *prairie* is a vacant lot; a *gangway* is a passage between two apartment buildings; *clout* is political influence.

formants of Scandinavian descent; it has not been picked up by other groups as it has in Minneapolis.

Negroes born in Chicago before 1900 vary less from their Caucasoid contemporaries than the latter do among themselves, attesting to something like genuinely integrated residential patterns in the past. However, Chicago-born Negroes under 50 show many features of Southern and South Midland pronunciation, notably in the consistent use of /sikágo/ in outland fashion, in often having /griz/ and /grizi/ as verb and adjective,<sup>13</sup> in the frequent loss of postvocalic /r/ in *barn*, *beard* and the like, in contrasts between *horse* and *hoarse*, and in relatively greater length of stressed vowels. Such extralinguistic speech traits as wider spreads than Inland Northern usage offers between highest and lowest pitch or between strongest and weakest stress, or the quaver of ingratiating when speaking to someone presumed to be in authority, also survive and are generally recognized by Chicago-born whites and identified as characteristics of Negro speech. In grammar, the Chicago-born Negro who grows up in an environment of poverty and limited cultural opportunities — as most Chicago Negroes grow up — has a tendency to use forms that identify him easily and to his disadvantage, in writing as well as in speech. Most of these are forms of common verbs — absence of the third-singular present marker, as in *he do*, *it make*; old-fashioned preterites and participles, as *help* “helped”; or the appearance of an -s marker in unexpected places as *we says*, *they does* — or in plurals of nouns, like *two postes* /-əz/. Many of these features of pronunciation and grammar, especially the lengthened stressed vowels, are also found among the recent immigrants from Appalachia, who have their own paralinguistic phenomena, such as strong nasalization, and a few grammatical peculiarities like the sentence-opening *used to*: “Used to, everybody in these-here hills made they own liquor.” But because the recently arrived Appalachian whites are relatively few in number, because their residential patterns are not so rigorously segregated as those of the Negroes, and because they are not so readily identified as to physical type, their linguistic features do not seem to be perpetuated into the younger generation, and probably will never be.

#### IMPLICATIONS FOR LANGUAGE ENGINEERING

As far as the teaching of English is concerned, the overt commitment of American education — whether or not it is always recognized, let alone successfully practiced — is that each student should acquire a command of standard English, the English of educated people, sufficient to enable him to achieve the social and economic position to which his ambition and intelligence and ability entitle him. This does not mean that everyone should talk like the works of Henry James or Walter Pater; it does mean that everyone should be aware that certain words or grammatical forms or pronunciations will tag them, justly or not, as unfit. We also have an American tradition—

<sup>13</sup> *Grease* and *greasy* have been studied by Atwood, 1950 and previously by Hempl, 1896.

again, one not always honored — of respect for the dignity of the individual and the integrity of the family, no matter how deviant their behavior by the standards of Madison Avenue or *Better Homes and Gardens*. No educational program should aim at forcibly alienating the individual from his cultural background; if he must make a break, he must make it with understanding of all the forces involved.

It would therefore seem in order for a language program to start from an examination of the data, probably of a much more massive collection of data than we have access to as yet. In fact, the gathering of data should be recognized as a continuing responsibility, for the culture and the language — and the values of cultural and linguistic traits — will continue to change.

With the data collected, it should be possible to determine which words or grammatical features or pronunciations are typical of the various social groups in the community. Once this objective social identification of speech forms is established, it should be possible to compare such forms with the common subjective reactions in the community, both as to the accuracy of the subjective identifications and as to the pleasantness or unpleasantness of associations.<sup>14</sup>

Forms, words, pronunciations which are obviously characteristic of a disadvantaged minority and which produce unfavorable reactions in members of the dominant culture should be the systematic target of the early programs in the schools. The emphasis should not be negative, on error-chasing exercises, but positive, on habit-producing drill.

Where the home language is something other than English, or is a variety of English sharply removed from the local standard, it would probably be desirable to teach standard English as a whole, as a foreign idiom, to be used in certain situations where the culture demands it. We would thus produce many bilinguals and bidialectals, capable of communicating with ease in two or more different cultural worlds. We have ample precedent: the late Eugene Talmadge of Georgia was the son of a plantation owner and won a Phi Beta Kappa key at the state university, but knowing that plantation owners and Phi Betes have little voting power in Georgia statewide elections, he perfected his command of rural Georgia folk speech to induce the "wool hats" to support him as their spokesman. Huey Long, on the contrary, was a "red neck" (poor white) from northern Louisiana, who had educated himself to a command of standard English (when he chose to use it) comparable to that of the Lodges and Saltonstalls. And those of us who have grown up in the South can cite many examples of Negro house-servants who — operating intuitively according to the classical tradition of decorum — could speak the folk dialect to the yard man and

<sup>14</sup> An instrument to evaluate such reactions has been prepared by Vernon S. and Carolyn H. Larsen, as a part of "Communication Barriers to the Culturally Deprived," Contract No. 2107, Cooperative Research Branch, U.S. Office of Education. A preliminary run showed that where the pronunciations of single words were concerned, Middle Westerners of the middle class could not distinguish between Negro and Southern white speech. Apparently the stereotype is to equate anything "Southern" with "rural" and "uneducated", for the Southern white whose voice was included in the instrument was the most highly educated and had lived only in urban areas.

cultivated English to the quality without making a lapse in either mode. It should be possible through education to give a larger number the advantages that a few have acquired through intelligence or luck or both.

Where traces of a foreign language pronunciation exist in a student's English, these should also be approached systematically by teachers who know the structure of the English pronunciation system, and if possible the system of the home language as well.<sup>15</sup> To linguists and other cultural anthropologists it is superfluous to remark that the problem is cultural, not physiological. Yet too often in the schools it is not recognized that the child whose parental language lacks the phonemic contrast between /s/ and /z/ constitutes a different kind of problem from one with a cleft palate.

English classes for speakers of other languages should be organized under professionally competent direction, with knowledge of what has been done recently to improve techniques and materials.<sup>16</sup> If there are many children from homes where a language other than English is spoken, special programs in English must be provided from the beginning of the child's school experience. Instruction toward reading English might be deferred until speaking and auditory comprehension are developed; instruction in reading the home language might be given instead. Conceivably, the Latin-American child could, by the fourth grade, be reading in Spanish two years ahead of what his Anglo competitors are doing in English and then could be more rapidly phased into reading English than if he had had reading thrust upon him before he had any competence in speaking or comprehension. Here, however, the design of programs is complicated, and the need for cross-disciplinary coöperation is particularly great.

An obvious difficulty in setting up special reading programs in the home language — perhaps in the long run more important than the lack of trained teachers or imaginative administrators — is the shibboleth of "segregation". But it is an established policy for the schools to provide special programs for children suffering from physical handicaps: the blind, the deaf, the lame. It would be equally humane to provide special programs for children suffering from cultural handicaps — a foreign language or a sharply divergent variety of English — which have some chance of being eliminated by intelligent diagnosis and purposeful instruction; no amount of talking though can restore sight to the blind. Furthermore, there is a great difference between genuine integration and mere physical juxtaposition; the earlier in the school career a positive language program is adopted, the sooner students will be able to perform as equals, regardless of race or ethnic background — the only situation in which true integration can be said to have been achieved.

Once the schools become aware of the need for specific programs to cope with the

<sup>15</sup> A series of contrastive studies, for English and specific other languages, is being published by the University of Chicago Press.

<sup>16</sup> The problem of preparing materials for special groups demands continuous experimentation and interdisciplinary coöperation. The needs of elementary school children in urban areas are least satisfactorily met so far, but any special group needs attention to the peculiarities of its learning situation.

problems of social dialects, two by-products for the school curriculum could develop naturally. First, the most obviously "culturally deprived" are those whose parents and grandparents, in the heat of the melting pot and the passions of war, were alienated from their native cultures and led to think of everything foreign as inferior. Depending on the size of the school system, there should be room in elementary programs for a broad spectrum of foreign languages, not merely those with the snob appeal of French, German, Italian, Spanish and now Russian, but little-discussed tongues like Croatian, Hungarian, Lithuanian and Ukrainian. To develop such a program would require cultural sensitivity and some intricate arrangements for shared time, but the cost would be relatively low and the potential gain in self-respect would be high.

The second by-product, even more easily achieved, and I like to think even more significant, is a deeper understanding of the meaning of dialect differences. Too many students, too many parents, too many teachers, shy away from alien varieties of English as from the plague; they feel that any variety different from their own is *ipso facto* inferior. In Detroit, even superior students have undergone the brain-washing of courses in "corrective speech" if their native pronunciation has been that of Oklahoma. But once the problem of social dialects is honestly faced, it should be possible to explain that differences arise not from mental or moral inferiority but from differences in cultural experience, and that the most divergent dialect, however ill suited for educated middle-class conversation, has a dignity and beauty of its own. Faced in this way, the social dialects of a metropolitan area become not a liability but an asset, a positive contribution to educating our students in understanding the variety of experience that enriches a democratic society.<sup>17</sup>

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<sup>17</sup> [The discussion of Dr. McDavid's paper was combined with that of the following paper by Dr. Labov. — Ed.]

## HYPERCORRECTION BY THE LOWER MIDDLE CLASS AS A FACTOR IN LINGUISTIC CHANGE

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A large part of our approach to language concerns the isolation of invariant functional units, and the invariant structures which relate these units to each other. Considerable progress has been made through this approach to the analysis of language, but in many areas we have reached a point where a different approach is required, in which the variable features of language become the primary focus of attention, rather than the constant features. The empirical study of linguistic variants shows us that linguistic structure is not confined to the invariant, functional units such as phonemes, morphemes or tagmemes; rather, there is a level of variant structure which relates entire systems of functional units, and which governs the distribution of sub-functional variants within each functional unit. This type of variant structure thus becomes a new type of invariant at a more refined level of observation.

The study of social variation in language is simply one of many aspects of the study of variant linguistic structures. One motivation for the linguist to study such structures is that they provide empirical evidence to resolve alternate structural analyses at the functional level, providing empirical solutions to problems which are otherwise meaningless. Secondly, variant structures are defined by quantitative methods which allow the detailed studies of linguistic changes in progress. The central theoretical problem with which the present report is concerned is the mechanism of linguistic change, in which the dynamics of social interaction appear to play an important part.

The process of linguistic change may be considered as having three stages.<sup>1</sup> In the ORIGIN of a change, it is one of innumerable variations confined to the use of a few people. In the PROPAGATION of the change, it is adopted by such large numbers of speakers that it stands in contrast to the older form along a broad front of social interaction. In the COMPLETION of the change, it attains regularity by the elimination of competing variants. In this discussion, the second stage will be of primary concern: at this stage, it appears that social significance is inevitably associated with the variant and with its opposition to the older form.

Social forces exerted upon linguistic forms are of two distinct types, which we may call PRESSURES FROM ABOVE, and PRESSURES FROM BELOW. By BELOW is meant "below the level of conscious awareness". Pressures from below operate upon entire linguistic systems, in response to social motivations which are relatively obscure and

<sup>1</sup> The scheme outlined here is that suggested by Sturtevant 1947, ch. VIII.



yet have the greatest significance for the general evolution of language. In this presentation, we will be concerned primarily with social pressures from above, which represent the overt process of social correction applied to individual linguistic forms. In this process the special role of the lower middle class, or more generally, the role of the second-ranking status group, will be the principal focus of attention.

The role of hypercorrection in the propagation of linguistic change was indicated in an earlier study on the island of Martha's Vineyard (Labov 1963a). Here a much larger body of evidence will be used, taken from my dissertation on social stratification of English in New York City (Labov 1964b). We will examine the hypercorrect behavior of a single class group in the speech community of New York City, and the consequences of this behavior for the process of linguistic change.

#### THE USE OF PHONOLOGICAL INDEXES

Most of the evidence to be presented is based upon the quantitative measurement of phonological indexes, although lexical and grammatical behavior is also considered. The methods of selecting, defining and measuring phonological variables, and of defining and isolating contextual styles, have been presented in previous reports (Labov 1964a; 1964b, ch. II, IV). These methods were applied in a survey of the Lower East Side of New York City, an area with a population of 100,000. The informants were drawn from a sample previously constructed by Mobilization for Youth, in a sociological survey carried out in 1961. A total of 207 adults and children were interviewed; most of the data to be presented here is based on the speech of 81 adults raised in New York City, whose speech was studied in the greatest detail, but information from several other sub-groups will be utilized as well.

The methods of quantitative analysis were applied to the problem of describing the phonological structure of the community as a whole, as opposed to the speech of individuals. Indeed, it was found that the speech of most individuals did not form a coherent and rational system, but was marked by numerous oscillations, contradictions and alternations which were inexplicable in terms of a single idiolect. For this reason, previous investigators had described large parts of the linguistic behavior of New Yorkers as being a product of pure chance, "thoroughly haphazard" (Hubbell 1950: 48; cf. Bronstein 1962: 24). But when the speech of any one person in any given context was charted against the over-all pattern of social and stylistic variation of the community, his linguistic behavior was seen to be highly determined and highly structured.

Such a structure of social and stylistic variation may be seen in Figure 1, showing the regular stratification of a single element of phonological structure. The phonological variable here is |th|,<sup>2</sup> the voiceless obstruent which is the initial consonant of *thing*, *through*, *thought*, etc. The vertical axis of Figure 1 represents the average

<sup>2</sup> In the notation used here, | | indicates a phonological variable, such as |th|. |th-1| indicates a particular value of the variable, and |th|-35 indicates an average index score for the variable.

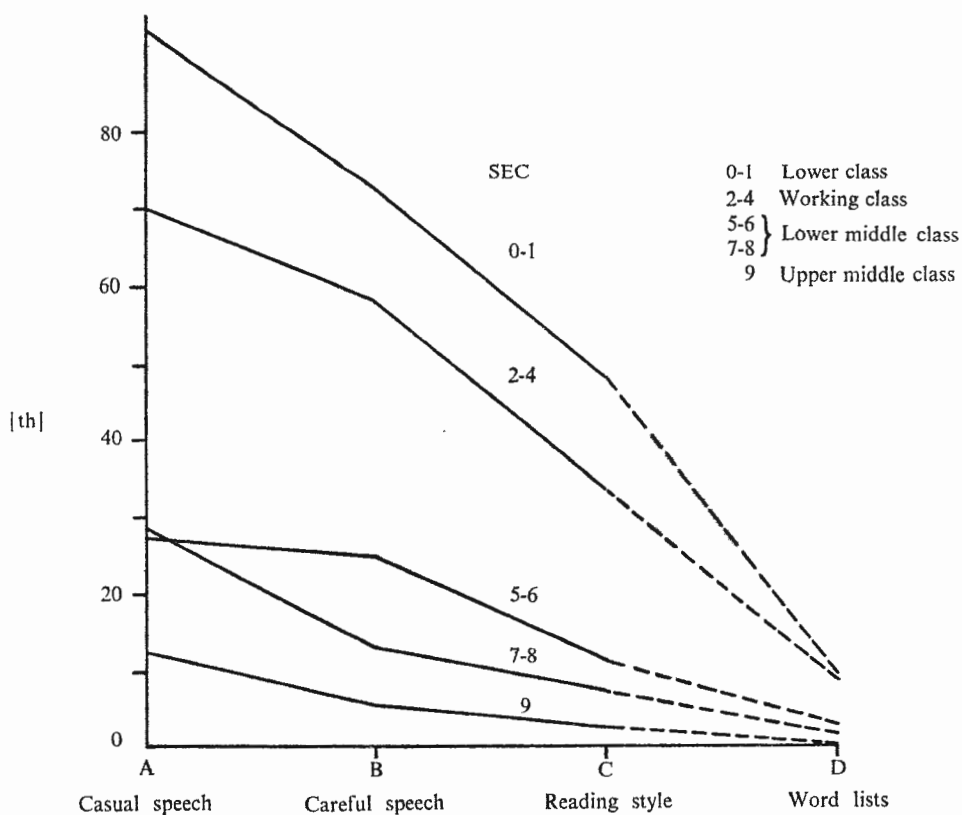


Fig. 1. Class stratification of [th]

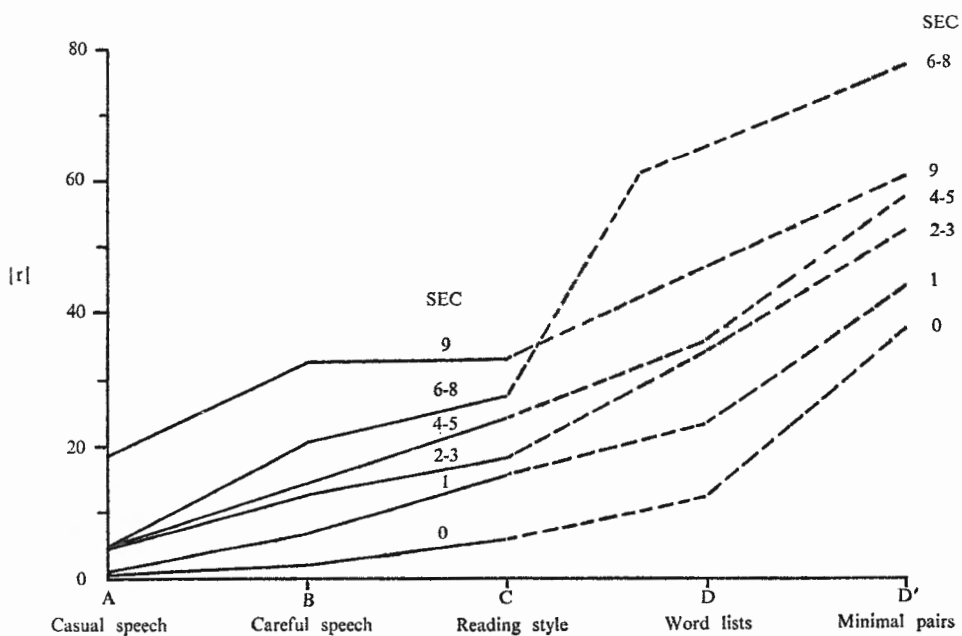


Fig. 2. Class stratification of [r]

index scores for [th], which are computed as follows: Each instance of the variable is given a numerical rating, 0 for the fricative, as in [aɪ θɪŋk sou]; 1 for the affricate, as in [aɪ tθɪŋk sou]; 2 for the stop, as in [aɪ tɪŋk sou]. The average value of these ratings, multiplied by 100, forms the index of Figure 1. The horizontal axis shows a series of four contextual styles, from the most informal at the left to the most formal at the right. At the left, at A, are the values for CASUAL SPEECH.<sup>3</sup> Next, at B, is CAREFUL SPEECH, the main bulk of the interview; at C, reading style for connected prose; at D, the pronunciation of isolated word lists. At each style, there is entered the index score for each of five socio-economic groups, and the scores for each economic group are connected along lines sloping from upper left to lower right. The socio-economic groups are divisions of a ten-point index of socio-economic status, derived by Mobilization for Youth analysts from the data of the primary sociological survey which preceded this linguistic survey. This socio-economic index combines three equally weighted indicators of productive status: the occupation of the breadwinner, the education of the respondent, and the income of the family.<sup>4</sup> Informally, we may describe these class groups by the terms shown at the right of Figure 1: group 0-1, lower class; 2-4, working class; 5-8, lower middle class; 9, upper middle class.<sup>5</sup>

Figure 1 shows a complex set of regular relations. Each value for a given class, in a given style, is lower than the value for the next most informal style, and higher than the next most formal style; it is also lower than the value for the next lower status group, and higher than the next highest status group (with one exception). From Figure 1, it is plain that the fricative form of [th] is the prestige form (as it is throughout the United States), and the stops and affricates are stigmatized forms. All class groups agree in their gradual reduction of the use of stops and affricates in more formal styles, and in each style there is clear-cut class stratification of the variable. In other words, we have a structure consisting of two invariant sets of relations.

#### HYPERCORRECT BEHAVIOR OF THE LOWER MIDDLE CLASS

In Figure 2, we see a somewhat different type of structure. This is a class stratification diagram for the phonological variable [r]. The vertical axis represents the average index scores for this variable: in the record of the interview, each occurrence of forms

<sup>3</sup> Casual speech is defined in the procedure when one of five situational cues occurs in conjunction with one or more non-phonological channel cues (cf. Labov 1964a).

<sup>4</sup> A detailed description of the derivation of this index may be found in Labov 1964b, ch. VII. Chapter VIII discusses the individual components of the combined index in reaction to linguistic behavior, and constructs a somewhat different scale for the analysis of some variables.

<sup>5</sup> The divisions of the socio-economic scale are those most appropriate for the stratification exhibited by the variable in question. Classes 2 and 5 are marginal groups, which sometimes show the linguistic behavior of the next lower group, and sometimes that of the next higher group. Core groups for the four classes are 0-1, lower class; 3-4, working class; 6-8, lower middle class; 9, upper middle class. A division 0-2, 3-5, 6-9 will show regular stratification for all variables (cf. Labov 1964b, ch. VII, Fig. 1).

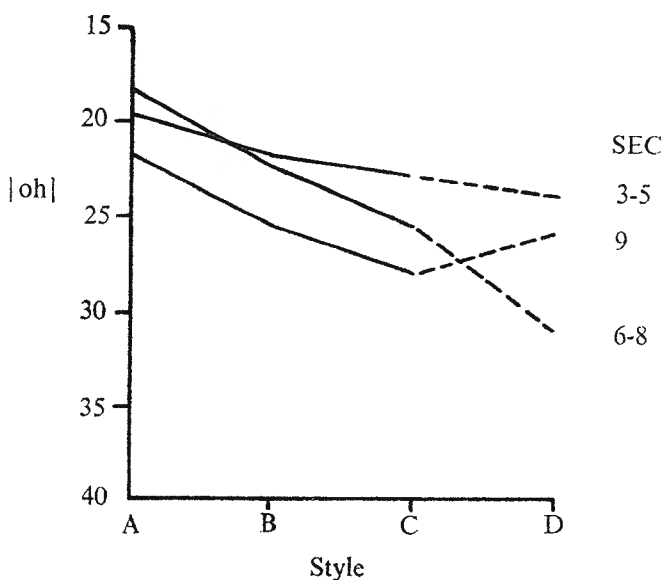


Fig. 3

with historical (or orthographic) *r* in final or preconsonantal position, is rated as follows: 1 for any type of constricted glide, 0 for unconstricted glide or no glide at all. Thus, in the sentence [tʃɑ:li nəvə kɛ:əd fə wə:m bɪ:], we would find six instances, all rated 0; in the sentence [tʃɑ:li nəvə kɛ:əd fə wə:m bɪ:] there would be six 1's. The percentage of 1's in the total number of occurrences yields the index.

The horizontal axis again shows the contextual styles, with the most informal on the left. At extreme right is one additional formal style, D'; this represents the pronunciation of minimal pairs in which |r| is the sole focus of attention, as in *dock* vs. *dark*, *guard* vs. *god*.

For each contextual style, the |r| index scores for six different socio-economic groups are shown; the general upward slope of the lines indicates the over-all status of |r-l| as a prestige marker in New York City.

At the extreme left, we see that only one class group shows any degree of *r*-pronunciation in casual speech; that is, in every-day life, |r-l| functions as a prestige marker of the highest ranking status group. The lower middle class, 6-8, shows only the same negligible amount of *r*-pronunciation as the working class and lower class. But as one follows the progression toward more formal styles, the lower middle class shows a rapid increase in the values of |r|, until at Styles D and D', it surpasses the usage of the upper middle class. This cross-over pattern appears to be a deviation from the regularity of the structure shown by the other classes, and in Figure 1. To describe this phenomenon, the term **HYPERCORRECTION** will be used, since the lower middle class speakers go beyond the highest status group in their tendency to use the forms considered correct and appropriate for formal styles.

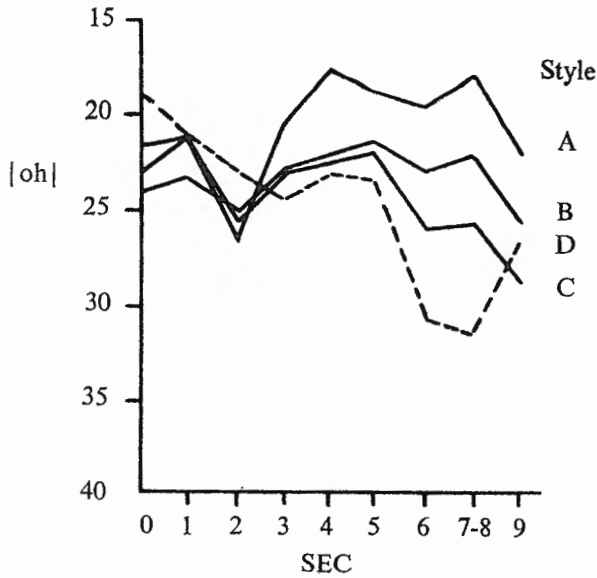


Fig. 4. Style stratification of |oh|

If the deviation shown in Figure 2 were found only in one case, it would be difficult to interpret — an irregularity in the method or in linguistic behavior. However, we find a similar cross-over pattern in several other structures. Figure 3, for example, is the class stratification diagram for |eh|.

This variable represents the height of the vowel in words of the class of *bad*, *dance*, *ask*, *half* — that is, monosyllabic morphemes and their derivatives which contain in other dialects a short low front vowel before voiced stops, nasals, and voiceless fricatives. The points on this scale are: |eh-1|, [bɪ:°d], with the vowel of *here*; |eh-2|, [bɛ:°d], with the vowel of *bear*; |eh-3|, [bæ:°d]; |oh-4|, [bæ:d], with the vowel of *bat*; and |eh-5|, [bæ:°d]. The average value of the ratings, multiplied by 10, gives the index along the vertical axis of Figure 3, which runs from 15 to 40. The horizontal axis shows the four contextual styles, running from casual speech at A to the reading of isolated word lists, at D. The class stratification shows four class groups, with the lower middle class crossing over the value for the upper middle class in the most formal style. That is, in reading a word list such as *bat*, *bad*, *back*, *bag*, *batch*, *badge*, the lower middle class speakers show the greatest tendency to say [bæt, bæ:d, bæk, bæ:g...], although in everyday speech, they are more likely to say [bɛ:°d], homonymous with *bared*: “I had a [bɛ:°d] cut; I [bɛ:°d] my arm.”

We find a similar hypercorrect pattern for the socially significant variable |oh|, in words like *caught*, *core*, *law*, *coffee*, *office*, *awful*, etc. In Figure 4, we have a STYLE STRATIFICATION diagram for this variable.

The vertical axis in Figure 4 represents the phonological index, formed from ratings which are parallel to those for |eh|: |oh-1|, [U:°f], with the vowel of *sure*; |oh-2|,

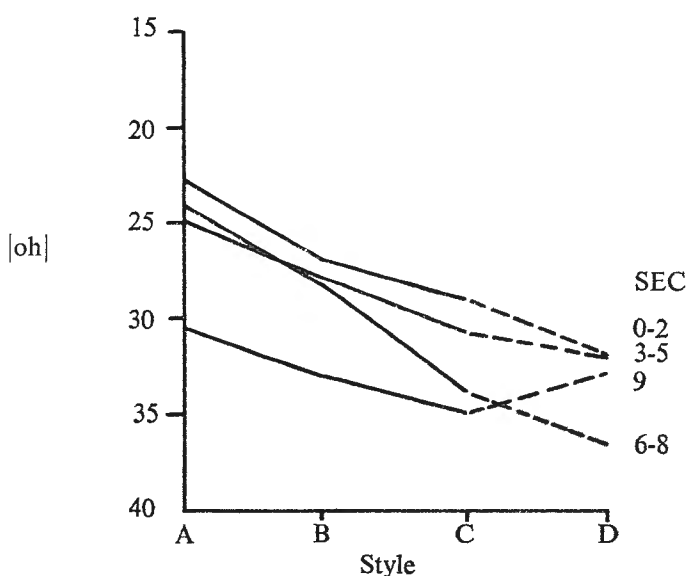


Fig. 5. Class stratification of [oh]

[ə:ːʔf]; [oh-3], [ə:ːf]; [oh-4], [ə:f]; [oh-5], [ɒf]. The average rating given to all occurrences, multiplied by 10, give the index scale running from 15 to 40 on Figure 4. The horizontal axis for this style stratification diagram shows the ten socio-economic classes. For each class, the value of the index in each contextual style is plotted on Figure 4, and the values for the same style are connected along horizontal lines. This diagram preserves all of the information present in the original data, and allows us to follow the differentiation of stylistic behavior in great detail. For the lower class, 0-2, the variable [oh] has no social or stylistic significance, as shown by the chaotic distribution of values in the four contextual styles. There is an intimate connection between stylistic and social variation, and as we see here, deviations from one axis of variation are usually accompanied by deviations from the other. The lower class shows no trace of the stratified pattern of the three higher ranking classes.

The upper section of the working class begins to show the high values of [oh] — [ʊ:ʔf] — in casual speech, values which are characteristic of the lower middle class as well. However, the social significance for the working class is not exactly the same as for the lower middle class, since the working class speakers show only a slight tendency towards correction of this vowel to more open values in formal style. The lower middle class, on the other hand, shows an extreme tendency towards correction, reading a list of words such as *Paul*, *all*, *ball*, *awful*, *office*, with a determined, though inconsistent effort towards [oh-5]: [pə:ːl, ɒl, ɒfəl, bə:ːl, ɒfɪs, dʊ:ːg...]. The highest ranking group, the upper middle class, shows a more moderate tendency in both casual and formal styles; as a result, we see the familiar cross-over pattern in Figure 5, which is the class stratification diagram for the variable [oh].

If we now compare Figures 2, 3 and 5, it is evident that the same hyper-correct behavior of the lower middle class is displayed. This deviation from regular structure is not, in fact, a deviation, but rather a recurrent aspect of a regular structure quite distinct from that of Figure 1. Figure 1 represents the structure typical of variables not involved in the process of linguistic change. A similar pattern may be found in the parallel case of [dh], the initial consonant of *this*, *that*, etc., or the morphological variable of the suffix *-ing*. On the other hand, the pattern of [r], [eh], [oh], is that of phonological variables involved in the process of linguistic change, as we shall see. The special role of the lower middle class in this pattern is apparent in the special sensitivity of this group to social pressures from above. We may gain further understanding of the behavior of this class by studying other types of evidence which go beyond the tabulation of objective performance.

#### HYPERSENSITIVITY OF THE LOWER MIDDLE CLASS IN SUBJECTIVE REACTIONS

In the study of the social stratification of language, we need not be confined to the evidence of objective differentiation of behavior. Social stratification has two aspects: differentiation, on the one hand, and social evaluation, on the other. In the survey of the Lower East Side, methods were devised to measure unconscious subjective reactions to individual values of the phonological variables (Labov 1963c; 1964b, ch. XI).

In the subjective reaction test (SR test), a tape recording of twenty-two sentences was played for respondents, who were asked to rate the speech heard in each sentence on a scale of occupational suitability. Only five different speakers were represented, all reading the same standard text, in which the values of the main phonological variables were successively concentrated. Since the respondent could not know exactly how he had marked any given speaker in earlier sentences in the series, each rating was effectively independent. It was therefore possible to compare the rating given to a speaker for a given treatment of a phonological variable with the rating given to that speaker in a ZERO SECTION which contained none of the variables. The effects of voice quality, reading style, etc., could therefore be cancelled out, and the unconscious reactions to values of a single variable were isolated.

In the test for subjective reactions to [oh], the respondents heard three different speakers use high vowels in the sentence, "We [ɔ:<sup>+</sup>weɪz] had [tʃɔ:<sup>+</sup>klɪt] milk and [kʊ:<sup>+</sup>fɪ] cake around four o'clock." If the respondent's ratings of all three speakers were equal or lower to ratings given by him to the same speakers in the zero section, his response was termed [oh]-negative. The percentage of respondents who showed [oh]-negative response for each class group was as follows:

Lower class	0-2	24 %
Working class	3-5	61
Lower middle class	6-8	79
Upper middle class	9	59

In this table, we see the subjective correlate of a number of objective patterns discussed before. The lower class, which did not participate in a pattern of social or stylistic variation for |oh|, showed no sensitivity to |oh| in the SR test. Secondly, there is here evidence of a very general principle which pervades all of the results of the SR test: that those who show the greatest use of a stigmatized feature in casual speech, show the greatest sensitivity to this feature in subjective reactions. Thus, for example, Italian working-class men, who show the highest use of stops for |th|, also show the greatest sensitivity to this stigmatized feature in the speech of others. Here, the working class, and particularly the lower middle class, who show high |oh| vowels in casual speech, show the most negative reactions to high |oh| in the SR test. Finally, we see that the lower middle class shows **HYPERSENSITIVITY**, in that it goes beyond the reaction of the upper middle class in its negative response to high |oh|.

A similar pattern emerges from subjective reactions to |eh|, differing from that of |oh| only in that we see a higher level of sensitivity on the part of the two lower classes: the following percentages of |eh|-negative response were shown by the four class groups:

Lower class	0-2	63 %
Working class	3-5	81
Lower middle class	6-8	86
Upper middle class	9	67

Here again we see the hypersensitive behavior of the lower middle class, which reacts more sharply to high |eh| vowels than the highest status class (and is joined here by the working class).

In the case of |r|, the lower middle class shows a level of sensitivity to this variable which goes beyond the reaction of the upper middle class, even though the behavior of the upper middle class in every-day speech is closer to the norm. The test for subjective reactions to |r| is described in detail elsewhere (Labov 1963c; 1964b, ch. XI): essentially, it measures the degree to which a respondent's reactions are consistent with the status of |r-1| as a prestige marker. The percentages of |r|-positive response for the various class groups again show the hypersensitive pattern of the lower middle class:

Lower class	0-1	50 %
Working class	2-3	54
	4-5	53
Lower middle class	6-8	86
Upper middle class	9	75

If this hypersensitive behavior is indeed associated with linguistic change in progress, and consistent with the findings on objective performance, then we should find that the lower middle class does *not* exceed the upper middle class in reaction to the variable



|th|. <sup>6</sup> The percentage of |th|-sensitive responses for the several class groups show that this is indeed the case, as there is no evidence here for a hypersensitive response of the lower middle class:

Lower class	0-1	58 %
Working class	3-5	76
Lower middle class	6-8	81
Upper middle class	9	92

So far, we have been considering evaluation of the speech of others. In another section of the linguistic interview, we explored the respondent's evaluation of his own usage of the variables. He was asked to choose which of four alternate pronunciations of a given word (*cards, chocolate, pass*, etc.) was closest to the way he usually pronounced the word himself. This self-evaluation test showed quite clearly that the extraordinary agreement in subjective response to the speech of others was not matched by an accurate perception of the respondents' own speech production. On the contrary, the respondents identified their own speech with those subjective norms which governed the direction of stylistic variation. For example, most respondents reported themselves as using variants of |eh| and |oh| which were lower than their own speech production, even in the most formal style.

It appears from this and other evidence that the New York speaker perceives his own phonic intention, rather than the actual sounds he produces; in this sense, the pattern which governs the direction of stylistic variation is determined by a structured set of social norms. It is phonemic, in the broadest sense.

The behavior of the lower middle class in this test is consistent with the hypercorrect pattern already shown. Lower middle class speakers show the greatest tendency to report very low values of |eh| — that is, they reported that they said [pæ:s] instead of [pɛ:s] — and by far the greatest tendency to report low |oh| vowels — claiming that they said [tʃɒklɪt] when they actually said [tʃɔ:klɪt]. On the other hand, the lower middle class showed no hypercorrect tendency towards inaccurate reporting in the self-evaluation of |th|.

#### LINGUISTIC INSECURITY OF THE LOWER MIDDLE CLASS

The great fluctuation in stylistic variation shown by the lower middle class, their hypersensitivity to stigmatized features which they themselves use, and the inaccurate perception of their own speech, all point to a high degree of linguistic insecurity for these speakers. Linguistic insecurity may be measured directly by several methods which are independent of the phonological indexes. One such method, depending on lexical behavior, shows an Index of Linguistic Insecurity (ILI) which is derived from

<sup>6</sup> For a description of the |th| test, see the sources previously cited.

a simple numerical procedure. The respondent is asked to choose between two variant pronunciations of eighteen words which have socially significant alternates, such as [ɑnt — ænt; lənθ — lɛŋθ; ɛskəleɪtə — ɛskjuleɪtə]. He is first asked to select the pronunciation which he believes is correct; then to select the one which he usually uses himself. The number of items in which the two choices differ forms the ILI.

The results of this measure show that the lower middle class exhibits by far the highest degree of linguistic insecurity in this sense. Table I shows the percentage distribution of scores for the four class groups, for four levels of the ILI. The lower middle class shows a much higher concentration at the higher levels of the index than any other group.

TABLE I  
*Percentage distribution of Index of Linguistic Insecurity scores by  
Socio-economic class*

ILI scores	Socio-economic groups			
	0-2	3-5	6-8	9
0	44	50	16	20
1-2	25	21	16	70
3-7	12	25	58	10
8-13	19	04	10	—
	100	100	100	100
N:	16	28	19	10

In the course of the survey of the Lower East Side, we also explored conscious attitudes towards language behavior. The attitudes of the respondents towards the speech of New York City, as expressed in overt, conscious discussion, reinforce the view that lower middle class speakers show the greatest linguistic insecurity.

In general, New Yorkers show a strong dislike for the sound of New York City speech. Most have tried to change their speech in one way or another, and would be sincerely complimented to be told that they do not sound like New Yorkers. Nevertheless, most of the respondents have been identified by their speech as New Yorkers whenever they set foot outside of the metropolitan area. They firmly believe that outsiders do not like New York City speech, for one reason or another. Most New Yorkers show a strong belief in correctness of speech, and they strive consciously to achieve such correctness in their careful conversation.

In all these respects, lower middle class speakers exceed all other New Yorkers.<sup>7</sup> The profound linguistic insecurity of the New York City speech community is most clearly exemplified in the conscious statements of the lower middle class respondents, as well as their unconscious behavior.

<sup>7</sup> With the exception that a number of lower-middle-class respondents stated that they had been identified by outsiders as NOT coming from New York City.

## THE ROLE OF THE LOWER MIDDLE CLASS IN LINGUISTIC CHANGE

We can now approach the question of linguistic change, particularly as it affects the phonological variables. In this discussion, the principal approach to change will be through internal evidence, in the distribution of linguistic behavior through the various age levels of the population. This distribution forms a dimension which we may call APPARENT TIME, as opposed to REAL TIME. In the complete report of the New City survey, the relations of apparent time and real time have been analyzed in detail, and the following cases considered (Labov 1964b, ch. IX):

- |                           |  |
|---------------------------|--|
| I. A stigmatized feature: | A. not in process of change                |
|                           | B. in process of change                    |
| II. A prestige marker:    | A. not in process of change                |
|                           | B. in process of change                    |
| III. Change from below    | A. early stage                             |
|                           | B. later stage, with correction from above |

Empirical data for examples of each of these types can be found, which in general confirm the analysis provided. In particular, the variable |th| corresponds to the case of a stigmatized feature not involved in change; |oh| to an early stage of change from below; and |eh| to a later stage of change from below, with correction from above. The latter cases are complicated by the fact that the principal dynamic does not lie in the area of class stratification, but rather in the contrastive behavior of ethnic groups. In the remainder of this discussion, we will consider the case of |r|, a prestige marker recently introduced into New York City speech. This variable is of course more than a simple phonemic alternation: to the extent that |r-l| is introduced, a whole series of shifts occur which reverse the main line of evolution of the New York City vowel system.

To begin with, we must consider the relations of real time to apparent time for a prestige marker in the process of change. For the highest ranking group, which shows the greatest linguistic security, these two dimensions match most closely. The oldest members of the upper middle class would tend to preserve their older prestige forms, as solidified relatively early in their development, and the younger members would show the adoption of the newer prestige form. When we consider the next highest ranking group, usually the lower middle class, the reverse situation prevails. The great linguistic insecurity of these speakers would lead to fluctuation in their norms for formal contexts, and even in middle age they tend to adopt the latest prestige markers of the younger upper-middle-class speakers. In this respect, they would surpass the younger members of their own group, who would not have had as wide an exposure to the structure of social stratification, and its consequences.

The working-class respondents might be expected to follow the same general lines of behavior as the lower middle class, but to a less pronounced degree. Only the lower class would be largely immune from the tendency to follow the latest prestige norms.

On the basis of these general considerations, we would expect the following scheme of relations between younger and older sections of the four class groups, as far as their use of the prestige feature is concerned:

	<i>Lower class</i>	<i>Working class</i>	<i>Lower Middle Class</i>	<i>Upper Middle Class</i>
Younger	low	[lower]	lower	high
Older	low	[higher]	higher	low

We can now consider the empirical data which corresponds to this diagram. Figure 6 shows the distribution of  $|r|$  values for casual speech.

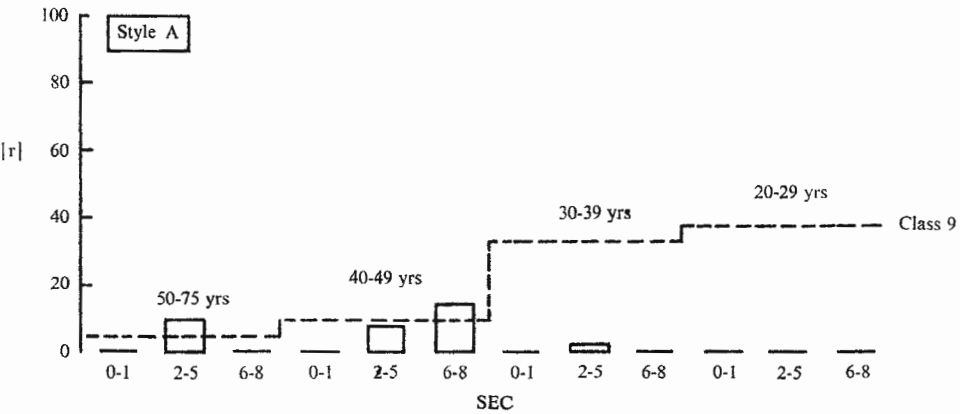


Fig. 6. Class stratification of  $|r|$  in apparent time: casual speech

The horizontal dotted line indicates the level of the upper middle-class use of  $|r|$ , and the vertical bars show the levels of the three lower ranking classes, for each age level. For the two older age groups, there is no indication of a social significance of  $|r|$ ; but for all those under 40 years old, there is sharp stratification, in which  $|r-1|$  has become the mark of the highest status group only. This sudden break corresponds exactly with a change in the social evaluation of  $|r|$ , as shown in Table II. While those

TABLE II

*Percentages of  $|r|$ -positive response to the two-choice test by class group and age*

	SEC						N:				
Age	0-1	2-3	4-5	6-8	9	All					
8-17	16	57	67	89	50	61	6	14	12	9	2
18-19	100	—	100	100	100	100	2	0	2	1	3
20-39	100	100	100	100	100	100	3	6	7	11	5
40-	63	67	50	70	57	62	8	18	8	10	7

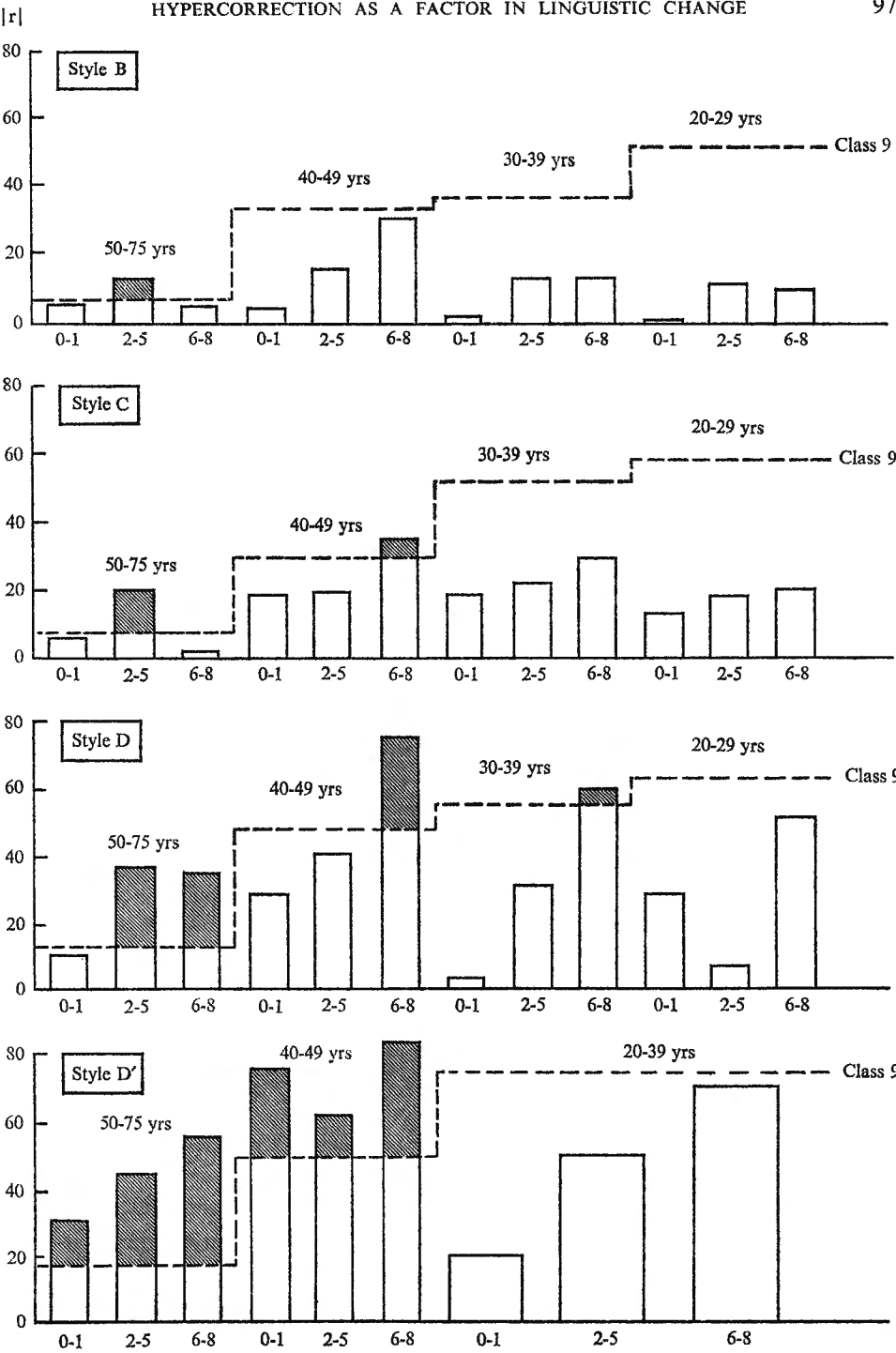


Fig. 7. Class stratification in apparent time for [r] in Styles B to D'

over 40 years old show a very mixed pattern in their subjective response to  $|r|$ , those from 18 to 39 show a complete unanimity in their positive evaluation of this prestige feature. Note that in the older group, the lower middle class shows the highest degree of  $|r|$ -positive response.

Table II has important implications for our general view of society as well as the development of language within society. We see the New York City speech community moving towards an increased diversification of speech performance (in Figure 6); but at the same time, in Table II, there has developed complete agreement in subjective evaluation. This sudden change, which coincides approximately with World War II, shows how social evaluation may precede and out-run the parallel development in overt behavior.

The data for successively more formal styles of speech appear in Figure 7. Beginning with Style B, careful speech, there is an increasing tendency for all class and age groups to use more  $|r-l|$ . However, it is the middle-aged members of the lower middle class group who show the greatest tendency to increase their use of  $|r-l|$  in formal styles, until in Styles D and D', they far surpass the level of the upper middle class. The hatched areas in Figure 7 represent the degree by which a given index score exceeds the level of the upper middle class.

Figure 7 is a complex structure, relating four variables. From top to bottom, there is a regular increase in  $|r-l|$  with increase of formality of contextual style; from left to right, there is a pattern of class behavior, repeated many times, in which the lower middle class leads the working class and lower class in the use of  $|r-l|$ . Finally, the larger left-to-right pattern shows a complex relationship of age level to  $|r|$ , in which the younger class 9 speakers show more and more  $|r-l|$ , while the reverse pattern holds for the correlation of age and  $|r-l|$  in the three lower classes.

With the background of all of the data presented in this discussion, the complex structure of Figure 7 seems comprehensible, and indeed, almost predictable. Without such a development of evidence, it would seem puzzling indeed. This was the case in an earlier survey of the use of  $|r|$  among department store employees in New York City (Labov 1963b; 1964b, ch. III). This survey made systematic use of casual and anonymous speech events to test the general hypothesis that any groups which were stratified by non-linguistic criteria would also be stratified by their differential use of  $|r|$ . Three large department stores were selected, from the top, middle and bottom of the price and fashion range; it was found, as predicted, that the use of  $|r-l|$  by employees was stratified in the same order as the ranking of the stores. But an analysis of the development of  $|r|$  in apparent time did not show the expected correlation: no over-all direction of change was seen. When the use of  $|r|$  was analyzed by both store and age level, however, a pattern did appear — the unexpected configuration of Figure 8. The highest ranking store, Saks, shows the expected increase of  $|r-l|$  with decreasing age. However, the middle ranking store, Macy's, showed the reverse relation, at a lower level. The lowest ranking store, Klein's, showed no clear-cut pattern. At that time, there were not sufficient data to account for this result. However, if we now

re-arrange Figure 7 in the form of Figure 8, we obtain Figure 9. Point by point, feature by feature, the two diagrams match. This convergence is the strongest possible verification of the findings of both surveys, for they approached the data by completely opposite methods. The possible sources of error are complementary: in each area where the department store survey was liable to error, the Lower East Side survey was most reliable, and vice-versa.

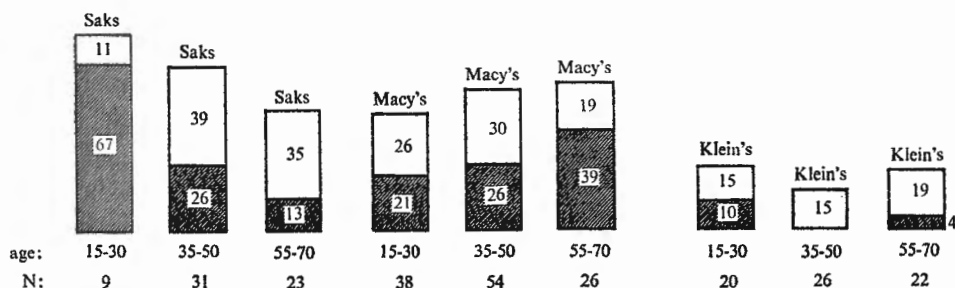


Fig. 8. Stratification of |r| in three department stores by age level.

Shaded area: % all |r-l|; unshaded area: % some |r-l|

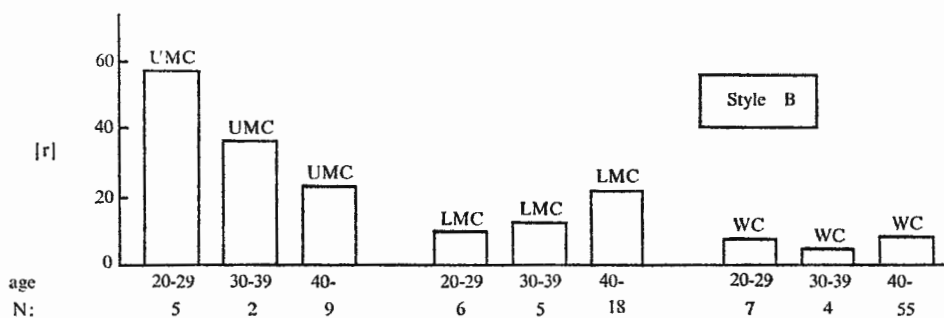


Fig. 9. Stratification of |r| by age and class on the Lower East Side: in Style B

The convergence of the two surveys establishes the validity of the analysis given above of the relations of apparent time and real time for a prestige marker which has recently been introduced into the linguistic structure of a community.

#### THE ROLE OF HYPERCORRECTION IN THE MECHANISM OF LINGUISTIC CHANGE

There is sufficient evidence to support the view that the introduction of |r-l| into New York City is indeed a relatively recent process, which showed a sharp qualitative increase in the years immediately following World War II. Traces of a similar development may be found in other *r*-less areas, but nowhere is the tendency so great as in New York City, and nowhere has it penetrated so deeply throughout the struc-

ture of the community. We can, of course, look to the influence of the broadcast media, where *r*-less patterns have all but disappeared, but this is a factor which affects all sections of the United States. In order to account for the special development of New York City, it is necessary to look to social and linguistic mechanisms which are peculiar to the metropolitan area.

New York City, as a speech community, may be regarded as a sink of negative prestige. This is not a recent pattern: the prestige dialect seems to have been a borrowed one for as long as we can trace it, and the process we are witnessing here is primarily one of substituting a northern-midwestern *r*-pronouncing dialect for the older *r*-less prestige dialect borrowed from Eastern New England. Yet the differences between the newer and the older forms are profound from the point of view of phonological structure. All of the New York City respondents had grown up speaking an *r*-less dialect: since they had acquired *r*-pronunciation long after their primary speech pattern had been established, it was not possible for them to achieve consistency in the use of  $[r-1]$ , even in the most formal context.

The evidence of this study as a whole indicates the following sequence of events in the genetic development of the complex linguistic structures that have been displayed here. The child's first experience in the use of English, at 2 to 3 years old, is usually dominated by the example of his parents. But from about 4 to 13 years old, his speech pattern is dominated and regulated by that of the pre-adolescent group with which he plays. These are the peers who are able, by their sanctions, to eliminate any deviations from the dialect pattern of the group. It appears that this pre-adolescent period is the age when automatic patterns of motor production are set: as a rule, any habits acquired after this period are maintained by audio-monitoring in addition to motor-controlled patterns.

It is in the first year of high school that the speaker begins to acquire the set of evaluative norms which have been displayed in this presentation. He becomes sensitive to the social significance of his own form of speech, and other forms; complete familiarity with the norms of the community seems to be attained at the age of 17 or 18. On the other hand, the ability to use prestige forms of speech, such as *r*-pronunciation, is not acquired until relatively late: the youngster seems to begin this process at 16 or 17. A working-class or lower-middle-class youth never attains the security in the use of this prestige form which the youngster from an upper-middle-class family does: as we have seen, even at the age of 30 or 40, the lower-middle-class speaker may be intent on changing his careful style, shifting his concept of the prestige norm to meet the most recent standards. In contrast, the college graduate has attained a certain degree of security in his use of English, partly through extensive contact with prestige speakers, and partly through the approval of his fellow students. Despite the fact that he may thus depend upon an acquired secondary prestige pronunciation, his use of this form may remain relatively constant from that time forth.

The problem which must be faced now is: how can such a mechanism lead to the solidification of *r*-pronunciation as a native speech pattern, for careful or casual



speech? The period when primary speech patterns are solidified is separated by a gap of at least four or five years from the period when secondary prestige forms are learned. The pre-adolescent may be influenced in his speech pattern by those who are one or two years older, but it is hard to imagine that he would be in close contact with those who are four or five years older. It would then seem that this mechanism can lead only to permanent stratification, and that *r*-pronunciation would never penetrate to the pre-adolescent period.

Yet one must consider that the original solidification of New York City as an *r*-less area must have followed the same pattern as that we are now witnessing, but in reverse. *R*-less speech was originally a prestige form, modeled on the fashion of Southern British speech, and the present configuration of *r*-less areas, surrounding Boston, New York, Richmond, and Charleston must represent the successful introduction of a prestige form into the primary native speech pattern. If the process was completed once, it can be completed again.

The key to this puzzle may lie in the hypercorrection of the lower middle class. We have seen that middle-aged, lower middle class speakers tend to adopt the formal speech pattern of the younger, upper middle class speakers. This tendency provides a feed-back mechanism which is potentially capable of accelerating the introduction of any prestige feature. Instead of a gradual, generation-by-generation spread of a feature from the highest ranking group to the lowest ranking group, we have here a means by which the process can be brought to an entirely different tempo. The lower middle class youth (and to a lesser extent the working class youth) is in contact with the new prestige pronunciation on two fronts. On the one hand, he is familiar with the speech of those who are going to college, whether or not he belongs to this group. On the other hand, his parents (and his teachers) also use this prestige pattern in the most formal circumstances. Normally, the dialect used by his parents has little obvious effect upon his own native dialect form: it makes no difference whether they come from Maine or Brooklyn, as far as his own speech is concerned. However, it is not impossible that repeated use of  $|r-1|$  by the parents in the earliest stage of language learning, may lay the groundwork for automatic and consistent *r*-pronunciation. Such an effect is not strong in New York City today, except perhaps in some upper middle class homes. But it may well be that in another half-generation or so, the use of *r*-pronunciation in formal styles on the part of adults may have increased to the point that children will acquire this pattern among their earliest, motor-controlled habits. One might think that parents would use only casual speech patterns in speaking to their young children; but on the contrary, I have heard respondents using the most careful, *r*-pronouncing forms when scolding their children. Since *r*-pronunciation has been adopted as the norm for the most careful type of communication, it has perhaps become appropriate for many types of interaction between parent and child. Hypercorrectness is certainly strongest in women — and it may be that the lower middle class mother, and the grade school teacher, are prime agents in the acceleration of this type of linguistic change.

The existence of the hypercorrect pattern in New York City has been established beyond any reasonable doubt. The suggested role of hypercorrection in the acceleration of linguistic change has been put forward with the expectation that further empirical studies may confirm or refute this possibility. Similar investigations may profitably be carried out in other cities, perhaps in those which do not show as great a range of stylistic variation in the speech community. Furthermore, it is necessary to explore more deeply the social motivation which underlies the more systematic and obscure process of change from below. All of these investigations will help in the illumination of the important problem of establishing the mechanism of linguistic change.

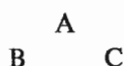
#### DISCUSSION

LABOV: The paper that I've submitted to the conference is a concrete example of the type of work that has come out of our studies on the language of New York City; these studies are in turn part of a more general program for the study of language in its community context. In this paper the more general principles are stated quite briefly, and I have given in some detail the evidence that pertains to one relatively narrow aspect of the results obtained so far. It might therefore be useful to spend some time on the general principles that have governed this work from the beginning.

The New York City studies might be considered an example of "micro-linguistic" as opposed to "macro-linguistic" investigation. I have worked on a small scale, examining behavior in a single speech community. Despite the fact that the community contains almost fifteen million people, it is still a context of limited scope compared to the national problems engendered by multilingualism in other countries. However, I think that many general principles can be inferred from this work. I do not know how universal these principles may be in application: that is one of the things I hope to learn through contact with those linguists who are working on a broader scale. My focus has always been on the understanding of linguistic change: the mechanism of linguistic evolution. There is one anomalous aspect of this problem which strikes us when we compare linguistic evolution with biological evolution. In botanical or zoological evolution, one can claim that the function and the structure of the organism are inextricably intertwined, and that functional change accompanies structural change. We normally consider that the primary function of language is the communication of information. The phoneme and the morpheme are often defined in terms of their communicative function, that is, their role in distinguishing utterances that have different cognitive content. And yet the evolution of languages as we have witnessed it, or as we infer it, seems to serve no cognitive purpose whatsoever. On the contrary, the diversification of language families seems to be non-functional in the extreme if we hold to our original assumptions. This means that either (1) we cannot depend upon analogies drawn from biological evolution, or (2) we have to reconsider the relative roles of cognitive communication and other forms of communication as far as the mechanism

of linguistic change is concerned. The second line of thinking appears in the work I am reporting here.

The analysis of linguistic structure on a synchronic plane is also one of our major concerns. A great many discussions of linguistic structure have led to moot alternatives, and some writers have come to expect such impasses as a matter of principle. Others feel that the resolution of alternative theories must lie in arguments from simplicity, or symmetry, or other such abstract considerations, rather than conformity with empirical evidence. The point of view which lies behind the work reported here is that two theories are not different in any meaningful way if they both conform equally to the observable evidence. Sociolinguistic study provides us with a rich new vein of evidence that helps us to resolve disputes which would otherwise remain moot and meaningless. Suppose, for example, that a phonological analysis proposed a triangular system of this type:



It might be argued that this is merely an arrangement of symbols on paper, and that we might equally well have a system in which A and B are parallel, and where C is the only low vowel. The distribution of variants within B and C should be helpful in resolving this question. If we find, for example, that the distributions of non-cognitive variants of the phonemes B and C are both concentrated at a higher point for the younger generation, as compared to the older generation (or for ethnic group Z as opposed to ethnic group Y), we have empirical evidence for the structural parallelism of B and C.

A great deal of the data discussed in my paper is composed of distribution patterns of this type. The methods of collection and analysis are designed to deal with CROSS-STRUCTURAL and SUB-STRUCTURAL VARIATION — and by structural, we mean pertaining to units defined in terms of cognitive function. Such variation can be traced across morphemic, tagmemic or phonemic boundaries. Phonological material has been our most useful base, because it can be most easily quantified. And the quantified indexes of phonological variation have led to the results presented here. The surprisingly complex regularities shown here have led me to the conclusion that linguistic structure cannot be confined to the arrangement of phonemes or features defined in terms of cognitive function only.

The fact that much of this variation is social variation, and not stylistic variation only, is not accidental. From the beginning of this work, and dating back to the beginning of my work on Martha's Vineyard, I have been unable to escape the impression that social pressures are continually exerted upon linguistic behavior, and have a continual, ever-present effect upon linguistic evolution. Social forces do not impinge upon linguistic development as remote catastrophes as Martinet would have us believe (in his view such catastrophes are rare events, like the Norman invasion, and their consequences are seen in a series of interlocking, internal structural oscillations

throughout the centuries). On the contrary, it appears to me more likely that the same social forces which have acted to induce language change in past centuries can be seen operating today. The question has been raised, again by Martinet, as to whether such causes are "internal" or "external" to linguistics. They are internal to linguistic systems in the sense that they operate whenever two people talk together, and so correspond to Martinet's definition of internal factors. On the other hand, our studies are bound to lead us to examine correlations with many aspects of non-linguistic behavior, and in that sense the investigation of social factors in linguistic change is a study of external effects upon the internal system.

These considerations lead to the general characterization of sociolinguistic research that is implied in the studies presented here. I think of sociolinguistics not as a new, semi-autonomous field that will develop its own principles, its own journals, its own methodology, but rather as a boundary area of research. It seems to me that sociolinguistic research is motivated by the need to disregard conventional academic boundary lines and the need to gather information on either side of those boundaries. We need this information, for example, in order to describe the evolution of linguistic systems. Each time that we search for a piece of information, even if it is the most technical kind of sociolinguistic detail, I think we should be fairly clear, as linguists, on its relevance to the problems of linguistic theory. Sometimes that is not easy, since one step leads to another, and the end is not always in view; yet I think that sociolinguistics is hardly an end in itself. Rather than respect the integrity of sociolinguistics, I believe that we ought to respect the central problems of the evolution of society and the evolution of language, of the structure of society and the structure of language.

In order to deal with the problems of linguistic evolution, it has been necessary to engage in considerable abstract analysis of the relationships of linguistic behavior to the dimensions of "apparent time" and "real time". "Apparent time" refers to the pattern of distribution of behavior through various age groups, while "real time" refers to the actual developments of the last five decades. To make this analysis, it has been necessary to distinguish two distinctly different types of linguistic change: those which occur in response to "pressure from above" and those which occur in response to "pressure from below". (Sometimes I refer to these more briefly as "change from above" and "change from below".) By "above" and "below" I mean above and below the level of conscious attention, not above or below a certain socio-economic level. In general, change from below is a systematic alteration of the dimensions of a linguistic system under the impact of an arbitrary social evaluation. In our data, it apparently originates in a particular ethnic or social stratum of the community, and gradually spreads to others in successive half-generations. Change from above is relatively unsystematic social adjustment in a linguistic system, in which overt social comment leads to the correction of individual forms. The gradual raising of /eh/ and /oh/ in the New York City vernacular through the past fifty years may be taken as an example of change from below. This shift appears to have originated in

the working class, spreading gradually to include other groups. The stigmatization of the pronunciation [əɪ] in words like *word*, *work*, *shirt*, and so on, is an example of an effective social correction operating on an isolated item. Under the effect of this change from above, the constricted *r*-pronunciation of this set of words has been restored even in the speech of those who use only *r*-less forms in all other word classes.

In the analysis of any given linguistic variable, we can begin by asking three basic questions: First, is the distribution stable through all generations, or does it show evidence of linguistic change? (For this question, of course, our most valuable data may come from earlier studies.) Second, if change is evident, is it an instance of change from above or change from below? Third, if it is an instance of change from above, under overt social correction, are we dealing with a stigmatized form or a part of a prestige pattern? The answers to these questions will enable us to proceed with the analysis of the linguistic change in view: one product of such an analysis is found in the paper presented here.

Finally, I would like to add a few comments on the relations between the linguistic structure we are studying on the one hand, and the community itself on the other. We have been surprised to find that New York City appears to be a single speech community, single in the sense that it is defined by uniform recognition of certain normative values in regard to language. Because New York City is a homogeneous speech community, we have been able to obtain the sociolinguistic structure of the city from a body of data based on the speech of 81 people, despite the inevitable noise that springs from personal variation and idiosyncrasies. I believe that it would be possible to complete surveys with 25 to 30 subjects, if properly selected, which would show us at least the basic pattern of class stratification in a population. We don't have to go to samples of two or three hundred: such large samples may have advantages in enabling us to complete extremely detailed analyses of linguistic changes, but they are certainly not necessary for a broad synchronic analysis.

We were surprised to find that New York City was united by a common set of shared norms, since it is diversified by radically different usage of some of the chief variables. Earlier investigations of New York City reported that speech was highly erratic, that people spoke one form or another by pure chance, and that the pattern was "thoroughly haphazard". If you simply enter the city, and listen to the speech of the people for a while, you would tend to sympathize with this point of view. But the basis for this mistake was that earlier studies were aimed at the 'idiolect' as the most consistent form of speech. Investigators thought that they would find the clearest, most coherent system in the speech of one person, and that two people would show a little less consistency, and that five people would show even more inconsistency. On the contrary, it appears that the speech of most individuals in New York City does not form a coherent, rational system by itself. That is, we will never be able to understand all of the variation in a person's speech if we study his speech alone. But if we place his performance against a background of a representative sample of 10, 15 or 20 of

his fellow citizens from the same speech community, we will find that his behavior follows a predictable pattern — not to the last detail, of course, but in its broad outlines. There will always be some factors influencing a given utterance which we have not accounted for; but if we have 5 or 10 utterances of a given speaker, the average index score will fall into a well defined place in the sociolinguistic structure of the city.

I believe that my paper has contributed evidence that the fundamental unit of linguistic structure in New York City is not the individual but the speech community. If that is so, I think that the same principle may apply equally to other speech communities, and yields a useful starting point for further investigation.

GUMPERZ: How much larger or smaller was your sample than the one used in the Linguistic Atlas sampling, and how did your sampling procedures differ from theirs?

LABOV: As far as I know, no one attempted to plan a dialect atlas to study social variation as well as geographic variation until Hans Kurath designed the Linguistic Atlas of New England. This seems an obvious step today, but it was a tremendous step forward in 1930. However, Kurath's principal aim remained the traditional aim of dialect geography: to trace the underlying pattern of regional differentiation in American dialects. In New York City proper the Atlas used 25 informants. These informants were selected according to the social criteria that are given in the New England handbook, admittedly informal, somewhat circular, and leaving a great deal to the judgment of the field worker. In a city like New York, it's obvious that the Atlas would not get as representative a sample as in a more rural area, where a judgment sample might be more successful. You will remember that the Linguistic Atlas required a very long interview, and convenience played a large part in selecting informants. It was particularly hard to get middle-aged informants, who were busy working during the day.

Our own approach in New York City followed the procedures of survey methodology; in fact, we utilized a stratified random sample already constructed for a sociological survey of the Lower East Side in 1961. Other parts of our work dealt with the city as a whole, but the most rigorous work was done with this Lower East Side sample representing 100,000 people and 33,000 households. It would not have been possible to construct as reliable a sample as this by ourselves: it took four months just to enumerate the population properly. As linguists, we didn't have the forces to begin such a program. Now these 33,000 households were originally sampled through 1,250 families that were drawn from 250 subsections or strata of 125 units; 988 individuals were interviewed in the sociological survey of 1961. This was the group that we had to work with.

In this section of New York City there are many foreign-born adults; of the 988, only about 500 were native speakers of English. Thirty percent of these had moved out of the Lower East Side in the two years that had elapsed since the sociological survey. (The problem of estimating the error introduced by this loss of potential subjects is discussed in detail in the references given in the paper.) We were then left

with 320 subjects who could be re-interviewed for their linguistic behavior: we chose by a random selection 195 of these as our target sample. Now we actually succeeded in interviewing 81 % of the 195 by one means or another — 157 people — in the course of three months' work with two interviewers. One hundred and twenty-two of these were the subjects of full-length interviews of the kind discussed in the paper. Of these 122, 81 were raised in New York City, 37 were raised outside of the city, and four had a divided upbringing. We were primarily interested in those who had spent their pre-adolescent years, from 5 to 13, in New York City and who had stayed in the city ever since. So these 81 subjects provided the main body of data on which the paper is based. Though 81 is a small number compared to the original 100,000, the sample is systematically derived by a method that eliminates most of the bias of selection, and allows us to estimate the errors produced by the remaining biases. Internal and external evidence leads us to think that our results apply generally to New York City, and that the patterns of social stratification which we studied are quite general. The sample of 81 that we used — and 81 was perhaps more than we needed — was large enough to reveal the basic pattern.

When I first started, I didn't think that ethnic differentiation would be important in New York City. I believed that the overriding influence upon speech, judging from the exploratory interviews, would be class stratification. For some variables this is the case, but for others, ethnic membership is important. In the sample of 81, 45 were Jewish, 27 were Catholic (of whom 19 are Italian), and 18 were Negro subjects. Now for the [r] index discussed in the paper, we obtained the same results for all ethnic groups; and for [th] and [dh], all ethnic groups also behaved in the same way. That is, all New Yorkers did so: the speakers who were raised out of town did not behave in the same way, despite the fact that some of the same ethnic groups were represented. But when we come to the [eh] and [oh] variables — the height of the vowels in the word classes of *bad*, *ask*, *dance*, and *off*, *talk*, *coffee* — ethnic group was more important than class membership in determining behavior.

MATHIOT: You mentioned casually that you had too many informants — more than you needed. Could you elaborate on that? And did you find out what would be the desirable sample? I mean, what are the reasons for reducing it further?

LABOV: This problem — the minimum number of informants that we needed — was always in view throughout our work in New York City. The result of our work is that we can now say about how many informants you would need for a particular correlation to be well defined. In the case of the class stratification of the main linguistic variables, we find, for example, that a sample of about 25 speakers is enough. In November of 1963, when I presented some of this material at Princeton, the records of only 25 informants had been transcribed and analyzed, and the pattern of class stratification was essentially the same as the one you see here. Therefore, if we were only interested in class stratification, a sample of 81 would have been too large. But if we only had 25 informants, I couldn't have said anything about ethnic differentia-

tion. With a sample of 81, we can study ethnic groups, age groups, and other independent variables correlated with linguistic behavior.

GARVIN: I have a general comment on the papers by Haugen, McDavid, and Labov. When one looks at the question of standard language, one can accept the three functions that Madeleine and I discussed some years ago [Garvin and Mathiot 1960] — the unifying function, the separatist function, and the prestige function — and then differentiate between the national prestige function, which consists of the prestige that a group feels it has from having a standard language, and the individual prestige function. If one does this, then there emerges a clear-cut distinction between the case of American English and all the other cases that have been presented: namely, that in American English apparently the most important function is the individual prestige function, whereas for Norwegians or some of the other groups that have been discussed, the unifying function, the separatist function, and the national prestige function seem to be overwhelmingly important.

LABOV: In listening to Dr. Gumperz' paper, I was struck by the shift that occurred between the local dialect and the Riksmål. The Norwegian case is similar in many ways to the one that we have in New York City, but the prestige pattern in Norway was the form that was used in the most casual context, in the most informal style. This is startling to someone who has been observing the reverse pattern, in which the socio-economic structure confers prestige on the middle class pattern associated with the more formal styles. One can't avoid the implication that in New York City we must have an equal and opposing prestige for informal, working class speech — a covert prestige reinforcing this speech pattern. The values that are covert in Norway are overt here, and vice-versa. We must assume that people in New York City want to talk as they do, yet this fact is not at all obvious in any overt response that you can draw from interview subjects. I think that we can tap the more covert subjective responses by different types of subjective response tests by using various ingenious devices. We can, for example, investigate the possibility that a social value of masculinity is attributed to the casual style of working class speech in New York City.

KELLEY: Do you have any control for the individual who would see himself as a masculine working class person and who would therefore give a high proportion of 'incorrect' responses, and who would be indifferent to this?

LABOV: We were surprised to find that 'middle class' values are so generally accepted by working class people where language is concerned. Because of this fact, it is hard for most working class people to feel secure about their own speech. One example can be seen in the use of stops or fricatives for [th] and [dh]. Italian working-class men use the highest percentage of the stigmatized forms in their casual speech: [dɪs tɪŋ, dæt tɪŋ, ɪ dɔɪ ʌdə tθɪŋ]. They are seemingly the least interested in abstract discussions of speech in general. But in the subjective response tests, those who used the highest percentage of a stigmatized feature in casual speech were the most sensitive to the use of this feature by others. In our tests, the listener rated speakers on a scale of occupational suitability: when he heard someone say "I suppose it's the same [tɪŋ]



with most of us", he would rate this speaker lower than someone who said, "I suppose it's the same [θɪŋ] with most of us", if he recognized the stigmatized status of [t] in this context. Now Italian working class men showed the greatest ability to recognize this feature, and to stigmatize the person who used it by placing him in a lower occupational category. And this pattern was seen all the way through the test. The fact that a person may covertly recognize other values does not prevent him from recognizing middle-class values in these contexts. The usefulness of the Index of Linguistic Insecurity is that it gives us an independent measure of the same kind of behavior: the recognition of a standard of correctness different from one's own speech.

The value of this index was shown quite clearly in the case of one man, a plumber of Italian working-class background. He was an example of 'status incongruence' because he earned a lot of money and paid a high rent, but his education, style of behavior and his occupation were more typical of working-class than middle-class men. This man gave minimal responses in many parts of the interview. He used a high percentage of stops for /θ/ and /ð/ in all styles (although he did show the normal direction of shift). His status incongruity was reflected most clearly in the index of linguistic insecurity, where he showed the highest index score of all: on 13 out of 18 items he said that the way that he pronounced the words was different from the correct way.

For most people, this pattern of insecurity was amplified at the end of the interview, by explicit denunciations of their own speech: "I don't talk well" — "Oh, this is awful, you don't want to listen to me." When they met a word like *aunt* or *vase* in a reading text, they would become confused and pronounce it in several different ways. However, the plumber that I mentioned didn't show this kind of behavior, and so it is possible that his index score shouldn't be lumped together with those of most lower middle class subjects. I may have two separate kinds of behavior classed as one.

LAW: Instead of 'insecurity' it might be worthwhile considering the term 'ambivalence' — between a value system and a pattern of actual usage. I would also like to refer to broadcast media as a possible influencing factor. Did you check out the possibilities that people would be changing their speech patterns because of the influence of television, radio, and movies?

LABOV: We have evidence to show that in New York City the influence of broadcast media is only one element, though it is certainly an important one. I think that the broadcast pattern is more symptomatic of a social change than a cause of one; broadcasting announcers are generally instructed to avoid all regional peculiarities. The change that has taken place is that *r*-less Eastern speech is now regarded as a regional peculiarity.

The pattern for *r*-pronunciation shown in Figure 2 is a very regular structure of social and stylistic variation. If you compare this with the same pattern for the 37 out-of-town informants, who were not raised in New York City, there is very little resemblance. Furthermore, in subjective response tests we found a sharp division among New Yorkers in their recognition of the prestige status of *r*. Those over 39 years old

showed no clear-cut trend, but those 39 years and under showed 100% agreement. Now the out-of-town informants were exposed to the same broadcast media in the last decade, but they don't show this differentiation at all. If anything, the older informants showed more recognition of the prestige status of *r* than the younger out-of-town informants, perhaps because they've been exposed to the New York City environment for a longer time. So the specific sociolinguistic pattern that we are observing here is peculiar to New York City.

GARFINKEL: Martin Orne, with his group, in their work on the demand characteristics of social psychological experiments, develops the fact that subjects come to a session anticipating it as an 'Experiment' that is being done in 'Science's' cause. Although subjects may be vague about that session's purpose, they have some ideas about 'Scientific Experiments' and their purposes. Without knowing specifically what this one's purpose is, they count it valuable and important nevertheless, and so they seek guides in performance so as to help make the 'Experiment' worthwhile. To accomplish such extra-experimental and tacit projects, subjects will search a session's features to learn what is being demanded of them, and will extend themselves to satisfy these demands.

Did you give any thought to the contributions that your subjects might have been making in an attempt to help you out? Is there any important sense in which we could say of your results that they would have been even better if you had told your subjects specifically what you were looking for, and then asked them straight out to please talk in the fashion to make it possible for you to write a publishable paper? I'm speaking quite seriously. In sociological interviews a respondent is frequently treated as if he were a judgemental dope about the conditions of the interview.

LABOV: We gave a great deal of time and thought to this question in our pre-testing. After all, I'm an *r*-pronouncer in the first place, and so is my other field worker. The pattern of *r*-usage that we observed in the informants could have been a social response that is quite general to a formal situation, but it also may have been the specific product of interaction between interviewers and informants. In fact, we did more than observe an unconscious bias towards *r*-pronunciation in our informants: we accepted this performance as a standard, characteristic of careful speech. Our pre-testing showed that this pattern appeared whenever we deliberately asked the informant questions and received answers. But when we got him excited to the point that his breathing came thick and fast, and the sweat came out on his forehead, and he gave nervous laughter, and seemingly forgot that we were there, or when he began to tell jokes and open a can of beer for us, then we felt we had evidence of a different situation, and the patterns of linguistic behavior confirmed our expectation. The question remains: how does this pattern compare with the pattern of every-day occurrences when the linguist is not present?

In the first place, we checked our results for casual speech against the results of observations of street games, casual conversations, public speeches, radio programs, and the like. Secondly, I conducted a separate survey of department store employees

that was independent of the presence of the linguist. The hypothesis tested was this: any groups of New Yorkers who are stratified in a certain order by non-linguistic criteria in accordance with the socio-economic hierarchy, will be stratified in the same order by their use of  $|r|$ . In this case, I approached department store employees (225 of them) in three well-stratified New York stores: Saks, Macy's, and Klein's. The subjects did not know that I was a linguist: they hardly knew that I had been talking to them. They didn't even know they were interviewed. I simply asked a question in a uniform context in which everyone was bound to use four words that contained the  $r$ -index, in the same way under the same conditions. The data was simple enough that I could record it in writing immediately after each exchange. Though I knew nothing about the personal backgrounds of these people, their occupational status, sex, and race were well defined, and their age could be roughly estimated. The pattern of stratification is the one that you see on Figure 8 of the paper, with the detailed breakdown by age and store. This pattern confirms in great detail the pattern that we obtained in our Lower East Side survey. The sources of error in the department store survey were exactly complementary to the sources of error in the Lower East Side survey, so that we can consider this very strong confirmation indeed.

GARFINKEL: Several of us have been working on features of very commonplace situations of interaction, trying to make them observable. For example, consider a commonplace conversation, like a conversation over a table, between people on the street, etc. There are features of that situation that are not usually observed by the parties to it, but which the parties are responsive to, that they count on as valued features of that situation. They are socially structured in the sense that you wouldn't have those properties if a person weren't somehow or other attempting to produce the kind of situation that has appropriate character for him. An obvious feature of this kind would be the common distances that people will stand with respect to each other. Over and over again we find that when you alter such a feature, by moving in closer for example, people get touchy and uneasy. They feel that SOMETHING is wrong. What it is, specifically, they don't know. The character of the situation itself is apt to have been altered in ways that are beyond their ability or their inclination to say. Now, I'm very interested in your notion of pressure from below, and I wonder if that also also involves structural features, properties of commonplace situations, that people use, but generally do not turn into topics of their own inquiry?

LABOV: Yes, I think so. I can give several examples of how these indicators give a precise record of shifts in behavior, beyond the conscious control of the speaker. I interviewed a Puerto Rican girl, who was beautifully poised, well dressed, and came close to standard English. When I asked her, "Where does your father live?" she said,  $[\partial: hi d\bar{3} liv w\bar{d} \Lambda s no m\bar{a}:]$ , and she used a much higher number of stops for  $/\partial/$  in the next few sentences than she had in the interview until that point. I would say that she was unable to concentrate her attention on the audio-monitoring necessary to preserve the fricative form. But you were talking about the more positive instances of using a speech form that has a marked social value. One of my informants is a

young Negro man who is now a vice-manager of an A&P store. He told me that he was once a member of a gang in Harlem that got into a lot of trouble, used narcotics, and so on. He had refused several times to join the group in some of their activities, especially when it came to smoking marijuana. One evening, he met one of the big husky members of the group outside his stoop, who asked him, "Why don't you get with it?" And he answered, very carefully, "Well, I just don't care to." And the other guy knocked him clear into the street. They were both conscious of the fact that his choice of each word, and his pronunciation of each word, was a deliberate insult. If he had just said, "Dad, I don't care for it," it would have been all right. Three days later, he was formally told to hang around somewhere else.

BRIGHT: I'd like to ask a question of Dr. McDavid. I remember reading some years ago statements to the effect that there was no such thing as a distinctive Negro variety of English, that Negroes spoke the speech of their economic class wherever they lived. Is this true of urban Negroes, or not?

MCDavid: This I wish I could answer. I think that we can't rule out the theoretical possibility that there may be some things in the paralinguistic or the like that doesn't occur in white speech at all, and that may be somatic rather than social in origin. I confess I am still skeptical of such a notion; but we can't rule it out. What I said about urban Negroes was that social selection — differential education, differential economic opportunities and the like — is likely to mean that more Negroes than whites, proportionately, belong to the lower economic and social stratum. This, in fact, would mean that although speech shows the effect of social conditioning, the effects of social conditioning would be more sharply manifested in Negro speech of the lower class than in the speech of their white analogues. All that you have to do is to look at the records of school attendance, school expenditures, reading experience, etc., and the evidence would certainly lead you to expect that more features of the so-called old-fashioned American English folk speech would survive in the speech of American Negroes than in that of whites, and in greater intensity.

BRIGHT: To take a specific case, are there any white groups who omit the third person singular present tense marker?

MCDavid: Yes, in East Anglia, in England, the omission of this marker is endemic. There are also a great many such groups, in Eastern Kentucky and elsewhere, among Southern and South Midland whites; see Bagby Atwood's *Survey of Verb Forms in the Eastern U.S.* [Ann Arbor, 1953]. But the crux is that by the time the Negroes get north, this omission of the /-s, -z, -əz/ marker, or its appearance in unexpected places, is identified by Northerners as a Negro speech feature, whereas it is not in origin. In the South it is not a Negro feature except to the extent that there is a larger proportion of Negroes than of whites who preserve more extreme varieties of old-fashioned speech.

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# TWO TYPES OF LINGUISTIC RELATIVITY (WITH EXAMPLES FROM AMERINDIAN ETHNOGRAPHY)

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## INTRODUCTION

LINGUISTIC RELATIVITY is a notion associated, via Whorf (1940), with the structure of language. To my knowledge, Whorf first proposed the term, using it to call attention to differences in linguistic structure, and to their importance for experience and behavior. Less studied, but, I think, theoretically prior, is a relativity that has to do with the use of language. The notion of a second type of linguistic relativity calls attention to differences in cultural pattern, and to their importance for linguistic experience and behavior.

The first kind of linguistic relativity is associated with inference from linguistic data to other aspects of culture. I propose to show that the second kind can be associated with inference from ethnographic data to functions of language.

To put the two types of relativity into perspective, I must comment on two theoretical questions. The first is that of emphasis on diversity vs. emphasis on uniformity; the second is that of the nature of the dependence between language and culture. In the other sections of the paper, I shall offer three kinds of example, drawn from Amerindian ethnographic literature. They have to do with contrasting situations of acculturation; controlled comparison, or genetic model; and patterns within a single culture. In neither the second nor the third case do the original data call explicit attention to the functions of language as such; both cases are sociolinguistic reconstructions, or explications, of conventional ethnographic accounts.

## RELATIVITY OF STRUCTURE AND OF USE: UNIFORMITY AND DIVERSITY

The recent history of the Whorfian hypothesis, and the prospective history of the thesis of this paper, must be understood against a background of assumptions as to uniformity (invariance) and diversity (variation), regarding the structure of language and the uses language serves. In principle, a theorist or period might be characterized by any one of a number of combinations: emphasis on uniformity of both structure and use, on diversity of both, on uniformity of one and diversity of the other. The actual historical picture is complicated by the need to take into account whether the

reference is intra-cultural or cross-cultural (to one system, or across systems). On this basis, there are altogether sixteen possibilities. Of these possibilities, the recent period in American linguistics and anthropology seems characterized by one, and the present period by emergence of another.

In the recent past, American linguistics and anthropology seem to have emphasized invariance of structure in analysis of a single language; variation of structure as between languages; variation in the handling of use with regard to a single language (the intrusion of data dependent on special functions being handled as deviation from a norm); and invariance of use as between languages. At the present time, there seems to be coming to the fore a pattern of emphasis which is the converse of that just described. There is emphasis on recognition of variation in the analysis of a single language; on invariance as between languages (universals of language); on invariance in the handling of speech functions in relation to a single system (language a 'system of systems', or sub-codes, with data dependent on special uses handled positively in terms of additional norms); and on variation of use as across languages. Part of this second pattern of emphasis is projected. Sociolinguistics seems to be rapidly establishing an emphasis on variation of structure in the analysis of individual languages; emphasis on variation of function across languages is the special concern of the present paper.

The patterns of emphasis just described can be highlighted in a pair of tables (Table I).

TABLE I (A)

	<i>Intracultural</i> (One-system)	<i>Crosscultural</i> (Across systems)
<i>Code structure</i>	Invariance (find the system)	Variation (relativity)
<i>Use</i>	Variation	Invariance

TABLE I (B)

	<i>Intracultural</i> (One system)	<i>Crosscultural</i> (Across systems)
<i>Code structure</i>	Variation	Invariance
<i>Use</i>	Invariance (find the system)	Variation (relativity)

It is a matter of emphasis always, not of exclusive prerogative for either uniformity or diversity, invariance or variation. Universals and concrete differences especially are interrelated, and the value of one does not cancel the value of existence of the other.

It may be necessary sometimes to correct an emphasis, or to expose the ideological role it plays. Thus, too ready an emphasis upon structural universals may reflect a wish for oneness and simplification in the world, and a tendency to overlook the level at which differences help and hinder individuals in the conduct of their lives.

With regard to the *Weltanschauung* problem, the important aspect of the recent pattern of emphasis is the cross-cultural part: the combination of emphasis on diversity of structure and uniformity of function (Sapir and Swadesh 1946 is an illustration; Sapir's magnificent essay on language, 1933, is a good illustration of emphasis on uniformity of functions). In part, I think, such an outlook has had the role of justifying the study of such languages as those of the American Indian, claiming scientific importance for them in terms of diversity of structure, and equality of worth in terms of uniformity of function. In part, such an outlook had a logical necessity, whether conscious or not. As linked to the *Weltanschauung* interest, the emphasis upon structural diversity had to assume functional uniformity. The very contrasts drawn in structure between, say, Hopi and English, presupposed for their cognitive and behavioral significance that the paired languages each played much the same role in the respective cultures. THE INFERENCE OF DIFFERENTIAL EFFECT ON WORLD VIEW ASSUMED EQUIVALENT ROLE IN SHAPING WORLD VIEW. (This is not to ignore that Whorf foresaw men transcending such a role through contrastive linguistics.)

Here lies the crux of the relationship between the two types of linguistic relativity. What if two languages do not have equivalent roles? It is the contention of this paper that the role of language may differ from community to community; that in general the functions of language in society are a problem for investigation, not postulation. (Subsequent sections offer some indications of differences in role.) If this is so, then the cognitive significance of a language depends not only on structure, but also on patterns of use.

To adapt Sapir's well known formulation (1929) of what Whorf designated as linguistic relativity (and used as epigraph to his article of 1941): we know that people who speak different languages do to some extent live in distinct worlds, not merely the same experiential world with different labels attached (although we are far from being able to explicate satisfactorily all that is involved in the qualification "to some extent"). Linguistic habits are in part constitutive of cultural reality. My contention is that people who enact different cultures do to some extent experience distinct communicative systems, not merely the same natural communicative condition with different customs affixed. Cultural values and beliefs are in part constitutive of linguistic reality. (For a three-way cultural contrast, see Mead 1937.)

#### RELATIVITY OF STRUCTURE AND OF USE: DIRECTION OF DEPENDENCE

Both Sapir's formulation (1929) and some of Whorf's most quoted statements seem to imply that, for the first type of relativity, language is the cause, and other aspects of



culture the result. The relationship between the two types of relativity might then seem to be one of difference in the direction of dependence. Both imply a degree of encompassment, but in the first, it is language which is viewed as matrix; in the other, it is the rest of culture. Or, in the first, it is the rest of culture which is viewed as channelled and embayed; in the other, it is language.

Such a bold sketch has a limited validity. An adequate picture of the relationship, however, must do justice to the actual complexity on both sides. In particular, one must do justice to the various strands that may be bunched together in association with the name of Whorf. The aim cannot be a general systematization of the Whorfian hypothesis (monographic treatments would be required for that; but cf. Trager 1959, Fishman 1960, Hymes 1961a, b). What is necessary for our purpose is to recognize that there are several levels, or steps, at which the subject dealt with by Whorf may be handled, and that each differs in its relationship to questions about the use of language. The three steps may be briefly characterized as concerned with (1) depiction and connection; (2) functional role; (3) nature and direction of dependence. (These steps are not the same as, but are not inconsistent with, the four dimensions distinguished by Fishman 1960.)

Step 1 is to show linguistic traits and to assume, assert, or show some significance for them as distinctive. Where but one, or a few, or scattered traits, especially lexical traits, are involved, the import may seem trivial (although it need not), and yet be cumulatively quite significant. (It is this step that Hockett and Lounsbury preferred in the discussion of the "Sapir-Whorf hypothesis" organized by Hoijer, 1954.) What excites more attention is the showing of a pattern, fashion of speaking, or style, among a number of traits. The important thing to observe here is how legitimate and ordinary such work is, and how little it says in itself as to the aspects of linguistic relativity that generate heat. To show a "fashion of speaking" or "cognitive style" is methodologically but an exercise in inference of pattern — a practice common to anthropology, and, in the first instance, essentially a form of typologizing. To infer the presence of a cognitive orientation, as one may be forced to do to make the evidence intelligible (e.g., consistent semantic shifts are hardly explainable except through a shift in cognitive orientation of some sort); or to point to analogous patterning in other aspects of a culture — these things do not of themselves entail any one position as to the source and status of what has been found.

To be sure, much work of this sort does assume or assert a specific role for language, and other work of the sort may attract attention because of the possibility of such a role for the language in question (such as that of shaping, or expressing, world view.) In such cases, however, the present level and that next to be described are joined, perhaps to the detriment of both (as I have argued at the conclusion of Hymes, 1961a.) Notice that the CONCLUSION of Whorf's major article (1941) is explicitly of the level being discussed; it finds connections between Hopi language and culture as historical products, but explicitly refrains from asserting diagnostic or necessary relations. The only causal assertion is the minimal one, namely, that the connections could have come

about only if the language and culture had been compresent for some length of time. While placed in the context of larger questions as to world-view and experience (as is Whorf's article before its conclusion), Hoijer's work on Navaho (1951) is of the same kind. It traces connections between selective emphases in the language and the rest of the culture — emphases, indeed, which insofar as they have to do with motion have been confirmed by other investigators.<sup>1</sup>

Work at the present step is compatible with a quite different larger context than that of world view. Sapir's statement that "Language is the most massive and inclusive art we know, a mountainous and anonymous work of unconscious generations" (1921: 235); and Newman's study of Yokuts vis-à-vis English (1940: 10), stating that "Each language is like a particular art form in that it works with a limited range of materials and pursues the stylistic goals that have been and are constantly being discovered in a collective quest. Yokuts is a type of collective expression that values balance of inner form and restraint in the representation of meanings", place linguistic and cultural patterns in the context of aesthetic form. Some European work has exemplified this approach.

In sum, the showing of pattern need make no one specific claim about significance in culture, other than the generic assumption that such significance exists. Such depictive work could remain exclusively isolationist in practice. It usually does not so remain, but in either case it entails the second step of the logic of linguistic relativity.

Step 2 is to assign a specific place to the linguistic traits or patterns. This may be done in terms of a restricted problem, or in terms of some general concern as to the place of language in culture. As we have just seen, quite similar depictive work may be quite differently placed.

The place assigned may be among any and all aspects of culture and social life: conscious or unconscious metaphysics, values, aesthetics, enculturation, productive activities, type or stage of culture, thought, behavior; or, the place may be one of effective autonomy. Whatever the case, assignment, though it may be implicit, is unavoidable. To take what is depicted linguistically as isolated from other aspects of culture is still to assign it a place within culture as a whole.

One puzzle in the history of the Sapir-Whorf hypothesis has to do with Sapir's own apparent change of view as to the place of linguistic patterns. In the oft-quoted passage from "The Status of Linguistics as a Science" (1929) and in the brief abstract, "Conceptual Categories in Primitive Languages", Sapir spoke in terms of "the tyrannical hold of linguistic form". Earlier, in his 1921 *Language*, he had made the Crocean remark quoted above, epitomizing language as expressive, a collective art; and he had distinguished the 'what' of thought from the 'how', taking the 'what', or latent content, of languages as the same, and treating the differences among languages as a matter

<sup>1</sup> Astrov, Reichard and Landar have variously found the same selective emphasis. It is strange to find Hoijer's article footnoted as an example of "untrammelled speculation" (by Lounsbury, 1963: 570). Untrammelled speculation is hardly an apt characterization of any of Hoijer's work.

of 'how'. Such a view seems to separate linguistic form (of the sort attended to in Whorf's essays on Hopi, for example) from the content of thought, and hence, from world-view. The role of linguistic form in shaping thought would be at best a minor one, more like a choice between heroic couplets and blank verse than a choice of unconscious metaphysics. The pervasive emphasis on the functional autonomy of language, especially of grammar, in the 1921 *Language* (and in an earlier essay, 1912) are equally hard to articulate consistently with the later view of the "tyrannical hold of linguistic form", which implies a tight integration between language and other aspects of life.

It may be possible to show the different statements to be consistent, by considering carefully the contexts of each, and by explicating them in terms of additional distinctions as to sectors of language and functions of language (e.g., distinguishing between the role of language in the development of a culture, and its role in the development of a child). Sapir did not provide such an explication, and the change remains, together with its significance for our present discussion. Sapir's presentation of diversity in grammatical categories in his 1921 *Language* surpasses Whorf's later essays in range and mastery, and his typology of linguistic structures in the same book is cognitive in basis (the fundamental criterion is conceptual type). The change, then, was not in attentiveness to diversity of form, the observation from which Whorf's linguistic relativity starts, nor in concern with depiction of pattern as having cognitive significance (the step 1 just discussed). The change was in the place given to linguistic form, from one of great autonomy to one of close functional integration. In other words, we have here within one man's thought an illustration of the independence of the second step from the first.

Degree of fit seems always in question here, along with place. Placement among some other aspects of culture implies some degree of fit for the linguistic traits. An isolated place still leaves open the possibility of some degree of congruence with other aspects of culture, due to some underlying common factor. (The possibility thus involves one among the positions that can be taken at step 3.)

All three steps, although analytically separate, are closely linked, and this is especially so for the second and third. It seems difficult to consider degree of fit without considering both a specific place for the linguistic traits (step 2) and the nature of the relationship that has given rise to the place (step 3). And, historically, few anthropologists have been content (as Kroeber was) with depiction. Most have been interested theoretically in language insofar as it raised problems both of fit with other aspects of culture, and of the causal relations to which the degree of fit was due. The 'language and culture' problem par excellence has been the connection between language and thought (nowadays, cognition); and social anthropology has its counterpart in the 'language and society' problem of the connection between kinship terms, social structure, and interpersonal behavior. The analytical distinction, however, still finds application; the two types of problem just cited provide an instance. Whorf ('language and culture') and Radcliffe-Brown ('language and society') may be said

to have agreed at the second step, in that each maintained that there was a close fit between linguistic pattern and non-linguistic behavior. With regard to kinship terminology, however, Radcliffe-Brown argued for a close determination of the linguistic by the non-linguistic, whereas at the beginning of his 1941 essay Whorf seems to argue for the reverse, and at the end of the essay, for neither.

As has been said, step 3 has to do with the nature and direction of dependence. More comprehensively put, step 3 is to interpret or explain the relationship between the data depicted (step 1) and the cultural place and fit found for them (step 2), as due to a particular kind of dependence between the two. Much discussion of Whorf, and of the general problem, has seemed to assume that some one causal connection was at issue. In point of fact, a large number of positions are both possible on systematic grounds, and attested historically. For present purposes, four major standpoints are considered:

- (1) language as primary (source, cause, factor, independent variable, etc.);
- (2) the rest of culture as primary;
- (3) neither as primary, the two being seen as jointly determining;
- (4) neither as primary, the two being seen as determined by an underlying factor (such as world-view, *Volksgeist*, national character, etc.).

It is essential to recognize that each of these four standpoints has a dual application. The Whorfian hypothesis comprises, not one, but two sets of relationships between language and that which language may determine. One set concerns the development of a culture, and another set concerns the development of an individual. In other words, there are both culture-historical (phylogenetic) and life-historical (ontogenetic) dimensions and the standpoint taken on one dimension can be independent of the standpoint taken on the other.

Let us take the alternatives for the cultural dimension as being designated by (1, 2, 3, 4) and take the four corresponding alternatives for the individual dimension as being designated by (a, b, c, d). There are sixteen combinations possible as positions on the "relation between language and culture" (1a, 1b, 1c, 1d, 2a, ... 4d). Some, to be sure, are unlikely (e.g., 1b, 1d). Among the positions adopted in the history of the topic are that of general cultural primacy (2b — Kroeber?); both jointly (3c — Whorf in some contexts?); primacy of some underlying factor (4d — W. von Humboldt?); primacy of the rest of culture on the cultural dimension, but of language on the individual dimension (2a — Boas? Sapir?); joint primacy on the cultural level, but of language on the individual level (3a — Whorf at the close of the article recurrently cited?).

Whorf seems to have expressed more than one of these possible positions in various contexts. Notice particularly that one may hold strongly to the constitutive, determining role of language in the experience of the individual (a), yet hold that language is a dependent variable, following the needs of culture, on the cultural dimension (2). Language habits determine, in the short run, cultural patterns and needs in the long run — such would seem the position taken by Boas. Notice also that it is possible to hold different positions with regard to different sectors of language (grammar,

vocabulary, phonology) in relation to different sectors of culture and behavior. Nothing is said of such matters here, but analysis along such lines (such as that by Fishman 1960) is essential, as a means of getting beyond global arguments as to THE relation between language and the rest of culture. Most or all the systematically possible positions may have to be coördinated as partial vantage points within an adequate theory.

The discussion of the three steps can be summarized as in Table II.

TABLE II

	(DO)	(ASSUME OR ATTEST)
Step 1 (Pattern)	Depict traits/pattern	some significance
Step 2 (Fit)	Place traits/pattern	degree of fit
Step 3 (Explanation)	Interpret traits/pattern	(See Below:) PRIMACY OF
		<i>Language/Rest of culture/Both/Neither</i>
IN RE Culture		1                  2                  3                  4
Person		a                  b                  c                  d

Let us return to the relationship between relativity of structure and of use, making use of the analysis just given.

Notice first that the three steps are more general in application than the emphasis on diversity associated with Whorfian concerns from von Humboldt (via Boas and Sapir) to Whorf. The same questions are compatible with an emphasis on uniformity. One might show patterns in languages, place them in respect to culture (and individuals), and interpret the relationship in terms of some underlying commonalty of the human brain, of human experience, or of both. Such an emphasis on uniformity would be compatible still with a view of language as a major determinant of individual experience and of cultural pattern. In short, linguistic determinism and linguistic relativity are logically independent.

A linguistic determinism emphasizing uniformity would do so partly by seeing uniformity where diversity had been thought to prevail. Still, such a view would tend to make language, or some sectors of language, less significant, by downgrading the facts of diversity that would still remain. In any case, either form of linguistic determinism leaves open the second type of linguistic relativity. Recently, Carroll, Greenberg, and others have described the Whorf sort of relativity as a wave on the surface of general

commonalty. This change of view has left the Whorfian assumption as to crosscultural invariance of function unchanged, except tangentially, through consideration of bilinguals.

Let us now consider how questions as to relativity of use arise in terms of each of the three steps, whether the linguistic determinism is relativistic or not. At the first step (depiction of pattern), the question of use need not arise; the significance claimed is only a generic one. At the second step (the place assigned to pattern, together with degree of fit), questions of use enter quite directly. For example, to place language as a collective aesthetic product, or as embodying a collective world view, need not be a matter of opposed theoretical position, but of use of a set of cross-cultural variables. Given a view of language that recognizes that it serves a number of functions, whose hierarchy may vary from case to case, it is not surprising to find one language serving predominantly aesthetic roles, and another language serving predominantly cognitive needs, or to find different aspects of the same language serving each.

More generally, given a view of language that approaches it in the context of the communicative system of a society, such differential allotment of function is only in keeping with the observation, as expressed by Mauss (1923), that "language is but one of the means of expression of collective thought and not the adequate expression of that thought itself." Scholars such as Jespersen have argued that the total range of conceptual functions served by a language may be generally the same (types of function such as the expression of number, space, time, person, etc., being universal), and that the differences among languages consist in the fact that the hierarchy and mode of implementation of such functions is not universally the same. From another standpoint, de Saussure observed that what is a matter of grammar for one language may be a matter of lexicography for another, so that a comparative or general theory must encompass both grammar and lexicography. So also for the total range of communicative functions served in a community. The range may be generally the same (types of function being universal), but the hierarchy and mode of implementation is not universally the same. What is a matter of one sector for one language may be a matter of another sector for another language; what is a matter of one language for one community may be a matter of another language for another community — even though the same language may be found in both. Further, language is but one semiotic system among others, and what is a matter of language for one community may be a matter of gesture, plastic art, ritual, for another. (Or, what is a matter of language categories in one case may be a matter of categories of the use of language — genres such as myth — for another.) In particular, metaphysical intuitions and assumptions may or may not be expressed in a language, depending upon the role the language plays in the community.

At the third step, the direction of the dependence between a language and an aspect of culture may vary with the age at which linguistic competence is achieved in regard to the aspect of culture; or, if the aspect of culture is not transmitted primarily linguistically, the relation between the age of general linguistic competence and that at

which the aspect of culture is experienced. Different sectors of culture are in general differentially involved with language, verbal art vs. hunting, for example; the degree to which language enters into socialization, ritual, etc., varies crossculturally; beliefs, values, and practices as to communication with infants and children vary; the scheduling of use of language varies in terms of settings and occasions for communication; etc. All the empirical conditions which govern whatever opportunity language has to play a role with regard to an aspect of culture may be found to vary cross-culturally. Explanation of the connection between linguistic pattern and the rest of culture cannot be abstracted from its ethnographic, sociolinguistic base.

From what has been said, the second type of relativity must suggest most readily the second of the choices possible at step 3, interpretation in terms of the dependence of language upon the rest of culture. In point of fact, all four major standpoints can be pertinent to it, just as well as to the first type. If we distinguish with Talcott Parsons a process of control (visualized as operating downward) from a process of conditioning (visualized as operating upward), then the cultural structuring of the use of language may be seen as generally controlling the role of language structure (standpoint 2); but the content of language may condition use, and on occasion decisively (standpoint 1). (Thus, in situations of acculturation, selection may operate on languages and their use in terms of the resources of the languages themselves; cf. Hymes 1961c: 78-9.) Sometimes it will be desirable to recognize language use and cultural patterns as both controlled by an underlying factor (or higher order factor, e.g., cultural values in a schema such as that of Parsons) (standpoint 4); and on some occasions, no useful purpose will be served by recognizing other than an interdependence (standpoint 3). In sum, with use, as with structure, a monolithic position is not tenable; an adequate theory must coordinate several standpoints.

Not only step 3, but the rest of the analysis into steps may apply to the study of the second type of linguistic relativity. Section (VI) will show as much. There a patterning of use of language is depicted, and some claim for general significance made; the pattern is placed in the context of a set of cultural events (when the events are considered in their larger cultural contexts, additional patterning appears); and an interpretation is made in terms of an underlying, or pervading, cultural valuation of the use of language.

The relationship between the two types of relativity should make apparent the need for a comprehensive ethnographic approach to the patterning of communicative behavior, including uses of language. I have tried to adumbrate what such an approach would detail in several papers (Hymes 1962a, 1964b, c, d). Here I shall illustrate one aspect of such patterning, the relation of cultural values to the place and use of language.<sup>2</sup>

<sup>2</sup> A general theory must depend primarily upon fresh studies from on-going cultures; but it must also be as extensively comparative as possible. Because of this, and because the data in question might otherwise be overlooked, as sparse and as not having been obtained with the theory in mind, I try to show that something can be gleaned from past ethnography.

## SOME SITUATIONS OF ACCULTURATION

In the study of bilingualism and acculturation, the type of relativity which this paper is concerned to stress is well recognized. There the notion that a language may occupy a distinctive functional place is indeed commonplace, as is the further notion that the particular uses to which each of the languages present is specialized should be associated with socio-cultural factors on which such use depends. In his admirable book on *Languages in Contact* (1953: 80, 87-8), Weinreich presents a useful set of categories for such analysis.

The main point I wish to make is that our awareness of such specialization in the use of a language, and our use of relevant categories, should not be restricted to multilingual situations. Conventional linguistic thought has suffered from a sort of schizophrenia in this regard. Specialization dependent upon sociocultural factors is recognized in multilingual situations, but when monolingual situations are considered, or are presupposed in general discussion of the functions of language, specialization dependent upon socio-cultural factors disappears from view. One way to overcome this inconsistency is to show that categories devised for dealing with multilingualism can be applied generally. I shall attempt this with regard to a particular set of categories, and a particular set of illustrative cases drawn from the New World.

The categories are a set of language functions devised by Czech students of standard languages. They have special interest for our purpose because they deal with socio-cultural values and attitudes. Four functions are distinguished, and three attitudes. A SEPARATIST and a UNIFYING function are correlated with an attitude of LANGUAGE LOYALTY. A PRESTIGE function is correlated with an attitude of LANGUAGE PRIDE. A FRAME OF REFERENCE function is correlated with an attitude of AWARENESS OF A NORM (Garvin and Mathiot 1960).

These categories were devised for analysis of a particular code in relation to others within a single situation, as generally is the case for categories devised for multilingual situations. We can assume, however, that formal, informal, and slangish modes of speech are found in all normal speech communities, and that normative evaluation of speech, together with differential individual competence, are universal. Given these assumptions, one can expect the functions and attitudes just stated to be potentially universal in their application. From the standpoint taken in this paper, the potentiality of universal application is indeed a test of the adequacy of such a set of categories. In general, a scheme of language functions should be adequate, not only contrastively within multilingual situations, but also contrastively across monolingual situations. If in a multilingual situation one asks WHICH language or code has a certain role, then in a monolingual situation one can ask WHETHER, or to what degree, language at all has a certain role.

There is a difficulty with regard to illustrations from the New World, or, indeed, from much of the rest of the world. One's evidence comes almost always from situations which today are multilingual to some degree. Particularly with regard to Ameri-



can Indian societies, one must consider the possibility that the present situation is due to the structure of the contact situation that has obtained in the past. So far as the cases to be considered are concerned, I do not believe that the interesting differences among them can be explained as wholly the result of acculturation. Rather, I believe that the differences argue for some differences in the place of language in the pre-contact cultures. This point is of some importance, since acculturational outcomes, as has been noted, are what we have to deal with for the most part. I am claiming that such cases can be used as evidence for differences in the role of language in societies of the hunting and gathering, and horticultural, sort that prevailed before European domination.

The illustrations consist of three contrasting pairs, one North American, one Middle American, one South American, taken in that order.

(a) The Hopi-Tewa have been an enclave on a Hopi mesa in Arizona since the early seventeenth century. Their Tewan language has had a clear and vital separatist and unifying function for them throughout that time, and has been associated with a consciously maintained prestige and pride. These roles of the language are bolstered by folk belief dating from the contact period, but bespeak a state of affairs antedating European contact (Dozier 1951).

In contrast, the Eastern Cherokee are a remnant group in North Carolina who retain their native language without evidence of unifying function or any apparent pride or prestige in its use. In his analysis of the case, Gulick (1958) reports that the knowledge and maintenance of the language is not tied to significant social divisions or community tasks. He expressly excludes aesthetic pleasure, such as verbal play or sound symbolism (in which the Eastern Cherokee show no interest), as a basis for the survival of the language; and he finds no evidence of its involvement in Whorf-like world view. He finds the language to persist only in a separatist function, associated with a purely negative, anti-White language loyalty. The situation must be due in significant part to the hardship and injustice of the acculturative experience, but the case suggests some pre-existing values and attitudes as well, since the very existence of the group stems from the determination of its progenitors not to be removed to Oklahoma somewhat over a century ago, and their successful evasion of all government efforts so to remove them. Comparative study of the role of language among the majority Cherokee group now in Oklahoma might clarify the matter.

(b) In Mexico the Zapotecs of the Isthmus of Tehuantepec retain pride in their first language and national identity, which are accorded prestige also by those around them. The Mezquital Otomi, a smaller group, are reported to have accepted an outside valuation of their language as inferior, and to feel no prestige in its use, although maintaining language loyalty (Wallis 1956). Differences in contact situation may have affected the present differences, but it is not a question of a large group (Zapotec) sustaining its culture better than a small (Otomi), for it is not the Zapotec as a whole, but particularly the Tehuantepec Zapotec of whom the special concern for language is true. A specialist in the study of the Zapotec (Laura Nader) considers the

role of language among the Tehuantepec group to reflect a pre-contact condition.

(c) The retention of native language among one South American group, and abandonment among another, seem to imply different roles for the languages in the pre-contact period (Hohenthal and McCorkle 1956). The Fulniô of Brazil have given up their lands several times during the last three centuries, moving in order to preserve their language and annual religious ceremony (to which proper use of the language is essential) as basis of their identity. In contrast, the Guayqueries of islands off Venezuela seem to have given up their aboriginal language and native religion so early that no trace remains. They have preserved their identity on the basis of a special socio-economic structure, as demonstrated by the fact that a small group who abandoned the special structure are today indistinguishable from other Venezuelans. Language, together with religion, has served a separatist and unifying function in the one case, but not in the other.

These cases can be put in a table (Table III). A 'plus' indicates that the aboriginal language has or had a major role in that particular regard, and is or has been significantly the object of the associated attitude; a 'minus' indicates that neither seems the case. Parentheses indicate less specific evidence.

TABLE III

	Zapotec, Hopi-Tewa, Fulniô	Otomi	Eastern Cherokee	Guayqueries
Separatist/Loyalty	+	+	+	—
Unifying/Loyalty	+	+	—	—
Prestige/Pride	+	—	—	—
Frame of Reference/ Awareness of Norm	(+)*	(+)*	(?)*	(?)*

\* For these groups, except perhaps the Fulniô, there is apparently awareness of a European language as a norm, but not as an object of loyalty or special pride. If one were to include Guaraní, as analyzed by Rona in his paper for this conference, one would score it 'plus' throughout on the level on which the other languages are being considered. On the level of its role as a presumptive national or standard language, a level which Guaraní shares with no other indigenous American language, it presently lacks a fully effective frame of reference function, and would be scored 'minus' there.

#### A CASE OF GENETIC MODEL: CROW AND HIDATSA

Crow and Hidatsa are languages still spoken in what is now the north central part of the United States, Crow in Montana and Hidatsa in North Dakota. The Crow were buffalo hunters, the Hidatsa horticultural village-dwellers. Linguistically the two groups were quite close, and formed a distinct subgroup within the Siouan family; whether their divergence is estimated glottochronologically or otherwise, it probably

amounted to no more than a few hundred years. Crow could learn Hidatsa with a little practice. The two groups thus exemplify what Romney (1958) has formulated as "the genetic model". The model consists in the use of a specifiable genetic relationship in language as a frame of reference for the reconstruction of other aspects of culture, or for their comparative analysis.

With regard to role of language, the Cherokee case discussed in the last section could provide an example of RECONSTRUCTION within the terms of "the genetic model". Were there appropriate data at hand for both groups of Cherokee, Oklahoma as well as Eastern, one could try to reconstruct the state of affairs that antedated the separation of the two groups in the first part of the last century. One would determine in what respects the two groups were alike, and in what respects different, with regard to functions and attitudes concerning language, and try to determine what portion of the resemblances and differences should be ascribed to subsequent acculturation, what portion to retention by one or both of antecedent conditions. (One might have to conclude that the Eastern Cherokee were a group self-selected with regard, among other traits, to role of language.)

The data known to me do not permit reconstruction of a common anterior state as to role of language for the Crow and Hidatsa; however, a discussion by Lowie permits use of "the genetic model" as a frame of reference for comparison of the two. In a limited sense the case is one of what Eggan (1954) and others have called controlled comparison in social anthropology, in which some features can be held constant so that features which differ can be better analyzed and understood. In the case of the Crow and Hidatsa what would seem to be constant is the bulk of the historically transmitted content of the two closely related languages. What would seem to be saliently different is the organization and mobilization of the content for use in the two cultures.

The discussion by Lowie (1917: 87-8) is a rare and fortunate example of direct comparison of two closely related cultures by a man who had investigated both firsthand. Lowie's discussion also exemplifies the problem facing a comparative ethnography of communication, if it is to make use of the available data on cultures now obsolescent or extinct. The problem, put in general terms, is to make explicit the implications of ethnographic statements bearing on the role of language. Put in terms specific to the present case, the problem is to identify instances in which the ethnography implies use of language. In the future it will be possible to draw on ethnographic descriptions undertaken with communication as focus, in which use of language will be stated as such. Most ethnographies to date must be explicated, if their contribution is to be realized.

I give now Lowie's account. The topics relevant to our purpose have been capitalized.

While probably every department of thought and customs presents some evidence of the former unity of these tribes, there are also noteworthy differences which may perhaps best be summarized by the statement that the Hidatsa enjoy a far richer culture than their western

congeners. Economically, they were not merely buffalo-hunters, but also hoe-agriculturalists and in connection with these features they inhabited, for part of the year, settled villages of earth-lodges. They were (and to some extent still are) conversant with the arts of pottery, basketry, and matting, and cross the Missouri in the oft-described bull boats, all of which features are unknown to the Crow. Ceremonially, the Hidatsa have an abundance of rituals associated with sacred bundles, to which there is likewise no parallel among the Crow. Finally, while the published mythological material from the Hidatsa is not adequate for a thorough-going comparison, it is surprising how often a Crow narrator gives to his tales a Hidatsa setting, the implication being that at least some of these stories were borrowed bodily from the Hidatsa stock of tales. That such transfer occurred in the case of certain dances and organizations is an historical fact. We may assume that much of the relative complexity of Hidatsa life is due directly to Mandan influence or at least indirectly to the stimulation received by contact with them, though we cannot trace in detail what was borrowed and what was transmitted as a result of this intercourse. The culture of the Hidatsa differs from that of the Crow not merely by the greater number and elaboration of discrete features but also in a marked trait of their social psychology — the tendency towards rationalization and systematization.

The Crow child, for example, SEEMS TO HAVE GROWN UP LARGELY WITHOUT FORMAL INSTRUCTION. Even on so vital a matter as the securing of supernatural favor, the adolescent Crow was NOT URGED BY HIS ELDERS BUT CAME MORE OR LESS AUTOMATICALLY TO IMITATE his associates: others had obtained benefits through visions, hence he also would retire into solitude in expectation of a revelation. With the Hidatsa everything seems to have been ordered and pre-arranged by parental guidance: THE FATHER REPEATEDLY ADMONISHED HIS SONS to make appropriate offerings and obtain the requisite ceremonial articles, at the same time GIVING THEM SPECIFIC INSTRUCTIONS.

THE DESIRE TO ACCOUNT FOR CULTURAL PHENOMENA is likewise very much more highly developed among the Hidatsa. Though the TALES accounting for the origin of the exoteric military organizations certainly do not show Hidatsa speculation at its best, they contrast favorably with THE BALD STATEMENTS OF TOTAL LACK OF STATEMENT by the Crow. In accounting for their sacred rituals, the Hidatsa, like the Blackfoot, largely draw on FOLKTALES for the incidents supposed to have led to the institution of their ceremonials. Nothing comparable was observed among the Crow: the origin of the chapters of the Tobacco order, for example, is associated with specific visions but NOT WITH A DEFINITE PLOT. It does not matter whether we assume that the Hidatsa tales were evolved in order to account for their rituals or were secondarily utilized for that purpose: in either case a distinctive trait must be recognized. They must be acknowledged to reveal, at least in this department of thought, either a superior degree of inventiveness or a superior capacity for coördinating and synthetizing disparate elements of their culture.

Associated with this tendency to rationalize and systematize there is naturally far greater rigidity, much less variability in individual interpretation and conception. The NAMES of the Crow clans, for instance, have clearly changed considerably in the course of time; those of the Hidatsa appear to have been immutable, there being no suggestion of other designations that those listed above [earlier in the monograph] and already recorded by Morgan. Versions of Crow MYTHS sometimes differ as widely as if they were collected among distinct tribes. Among the Hidatsa I think there is far more conformity to type, and though my experience in taking down their STORIES in the original is very limited I received the impression that even in the PHRASEOLOGY employed there was a markedly greater tendency to preserve a TRADITIONAL FORM. This seems to me to hold quite generally. If one inquires whether Old-Man-Coyote and the Sun are identical or whether Old-Woman's Grandchild was ever addressed in PRAYER, one receives diametrically contradictory replies from equally trustworthy Crow

natives, which is hardly likely to occur in parallel Hidatsa instances. Even in the application of KINSHIP NOMENCLATURE, where a certain fluidity is probably universally found, the Hidatsa are more consistent than the Crow, as may be shown by comparing the applications of the grandchild term and the designations of the mother's mother's brother. It is true that my personal acquaintance with the Crow is much more intimate than with the Hidatsa and that any expression of opinion on the latter must be weighted accordingly. Nevertheless an impression of contrast so definite and apparently so abundantly supported by concrete facts can hardly be without an appreciable element of justification.

The topics singled out by Lowie can all be seen to entail use of language: formal instruction; tales and myths; clan names; and kin terms. The first two topics could be characterized in a formal account of a culture as linguistic routines, i.e., patternings of the use of language beyond the level of the sentence. The second two topics are types of nomenclature, whose variation in use is especially noted.

An overall view of the linguistic dimension of the contrast between the Crow and the Hidatsa can be gained by placing in a table (Table IV) parts of the statements that bear on the topics (capitalized in the quotation). Terms which entail the use of language are in italics in the table.

TABLE IV

CULTURAL TRAITS	HIDATSA	CROW
1. <i>Formal instruction</i>	"everything seems to have been ordered and prearranged by parental guidance"	"seems to have grown up largely <i>without formal instruction</i> "
(a) Supernatural	"father repeatedly <i>admonished</i> ... specific <i>instructions</i> "	"not <i>urged</i> " ... "came more or less automatically to imitate"
2. <i>Accounting for Cultural Phenomena</i>	"very much more highly developed"	
(a) Military Organization	" <i>tales</i> ..."	" <i>bald statements or total lack of statements</i> "
(b) Sacred rituals	"draw on folk- <i>tales</i> "	"nothing comparable" ... " <i>not ... definite plot</i> "*
3. Individual Interpretation and Conception		
(a) <i>Names of clans</i>	"appear immutable"	"changed considerably"
(b) <i>Myths</i>	"far more conformity to type"	"versions ... differ as widely as if ... among different tribes"

\* It is not reported here whether or not the visions associated with the origin of the ceremonial order among the Crow contained verbal instruction.

- |   |   |   |
|---|---|---|
| (c) Religious concep-<br>tions and prac-<br>tices | “hardly likely to occur” (see<br>Crow column) | “contradictory <i>replies</i> ”<br>(as to whether two <i>su-<br/>pernatural figures</i> same,<br>one <i>addressed in prayer</i> ) |
|---|---|---|

4. Kinship *nomenclature* “more consistent” — (e.g., GrCh, MoMoBr)

As his statements indicate, Lowie himself refers the several specific contrasts to a general contrast between the Crow and Hidatsa in values and attitudes. He introduces the contrasts as showing among the Hidatsa a “tendency toward rationalization and systematization”, and, during the presentation of the contrasts, he says of the Hidatsa, first, that “the desire to account for cultural phenomena is likewise very much more highly developed”, and, second, that

They must be acknowledged to reveal, at least in this department of thought (rationalization of rituals by tales), either a superior degree of inventiveness or a superior capacity for coördinating and synthetizing disparate elements of their culture.

Lowie’s treatment of the Crow-Hidatsa differences shows that the relativity of the role of language must be considered at two levels. One level is that of such differences as have been described, which “can all be restated as a general difference between the two groups in the role and evaluation of speech” on the part of the groups themselves (Hymes 1962b: 902). A second level is that of differences as to bases of evaluation on the part of observers.

Lowie takes the Hidatsa traits as evidence of a superiority. His focus on the positive character of the Hidatsa traits, and the language of his phrasing, imply as much. One might, however, infer only differences in values and attitudes, not difference in capacity and merit as well. One could consider the Crow and Hidatsa as having cultivated different sets of traits, each set equally adaptive, but to different contexts. Even if practice and reinforcement were held to make the Hidatsa superior in effective capacity with regard to the traits in question, one still might find the basis for evaluating the traits too limited. On other bases of evaluation, indeed, one might take the Crow traits as evidence of superiority. Instead of describing the Hidatsa traits in terms of inventiveness and systematizing, for example, one might describe them in terms of compulsive ideologizing. Instead of describing the Crow situation as less developed, one might describe it as less trammelled. Instead of valuing the Hidatsa for synthesizing and coördinating, one might value the Crow for permitting greater freedom of expression, from the standpoint of individuals, or for providing a greater reservoir of variation for adaptive change, from the standpoint of society. One might even come at the contrast in Buddhist terms and find the Hidatsa more of a Hinayana type, the Crow closer to a Zen variant of a Mahayana type; on those terms, many would prefer the Crow.

In sum, there are at least two broad bases of evaluation—one having to do, roughly, with cognitive capacities; another, roughly, with freedom of individual experience.

Lowie does mention the second, but only to characterize greater rigidity, less individual interpretation and conception, as natural correlates among the Hidatsa of the tendency to rationalize and systematize. He evaluates (using the terms "superior", "contrast favorably") only on the dimension of inferred cognitive capacity. We can accept the adequacy of his description, but not of his bases of evaluation. That the Crow and Hidatsa differ is clear; that either term of the difference is superior is not. In pursuing this sort of study, we must be especially careful to examine our comparisons, and descriptive terminology itself, for bias and adequacy. (Notice that the Crow and Hidatsa seem to differ with regard to the functions and attitudes considered in the preceding section. Lowie's account can be taken as evidence of stronger development of the frame of reference function, and its associated attitude, awareness of a norm, among the Hidatsa.)

#### AN ETHNOGRAPHIC ACCOUNT: WISHRAM CHINOOK

In this section the relativity of the role of language will be analyzed in terms of comparison among portions of a single culture. In aim, the method is structural, but in execution, it must also be philological. Using what data exist for a culture no longer practiced, I shall try to infer something of the structural relationships which characterized its use of language, and of a cultural valuation of language implicit in such use.

The culture in question is that of the Wishram. Their village on the northern side of the Columbia, in the present state of Washington, was the easternmost extension of the Chinookan-speaking people who dominated the river for two hundred miles to its mouth at the Pacific. The data to be adduced are almost wholly from a monograph which represents 'memory ethnography' obtained on separate occasions by Leslie Spier and Edward Sapir early in this century.<sup>3</sup> Both men were excellent ethnographers, conversant with the cultures of the area, and Sapir obtained some of the material in native text. Use is made of some additional observations by myself and by David French since 1950.<sup>4</sup> None of the data was obtained with the questions now to be asked of it in mind. The relevant facts, nevertheless, are mostly clear and to be accepted with a certain confidence. What is said about the relationships among the facts, however, and about the underlying values, must remain largely an interpretation. The interpretation can have a good deal of plausibility, and be somewhat corroborated by different lines of evidence, but it cannot be given empirical test.

In such circumstances, as Lounsbury has observed, one cannot validate structural method and theory, but one can apply it. Future field studies, directed to such patterns in the use and valuation of language, can establish their general existence. I

<sup>3</sup> Page numbers in this section refer to Spier and Sapir 1930. See also Sapir 1909 (cited as WT).

<sup>4</sup> French 1958, 1961. Use is also made of field notes of David and Katherine French, and of linguistic data in the files of Walter Dyk, David French, and myself. The principal Wishram authority is Philip Kahlamat. David French has kindly read the first draft of this paper and made a number of helpful comments.

believe this will be done so firmly that we will have no doubt that ethnographic records, such as those for the Wishram, contain the partial manifestations of such patterns, just as now we do not hesitate to find in Sapir's Wishram texts the partial manifestations of the underlying system of a language. Thus, the proposition that Wishram culture had SOME patterns of the sort to be inferred here can be given theoretical verification. The proposition that Wishram culture had the PARTICULAR patterns to be inferred here must remain a cumulative probability, based on varied and partial data. The patterns are presented here primarily as an illustration of the possibility of a type of approach, and of the general theoretical point of the paper. Ethnographic research among the few surviving Wishram and Wasco (close relatives of the Wishram), and among neighboring Sahaptin-speaking peoples by French, may add data and insight. Subsequent theory may sharpen inference.

*The Speech Communities.* Let us consider first the speech community within which the use of language occurred. By external criteria, there was one, constituted by knowledge of Wishram Chinook as daily language. The community might be taken to include the trivially different dialects of neighboring Chinookan-speaking villages, such as that of the Wasco almost directly across the Columbia. In terms of Wishram culture, however, there were three speech communities, each marked by a distinctive code. The community constituted by knowledge of Wishram consisted of normal adults and of children past babyhood. (Babyhood was characterized as lasting "until they could talk clearly", 218, and training for guardian spirit experience began "when he can talk plainly" — six years being the earliest age, 239.) According to Wishram belief, Wishram itself was a SECOND language for everyone, including those born into the society. Wishram were born first into another speech community, one comprising babies, dogs, coyotes, the guardian spirits named Dog and Coyote, and old people possessing those guardian spirits.

Such guardian spirits could understand the language of babies. They maintain that a dog, a coyote, and an infant can understand each other, but the baby loses his language when he grows old enough to speak and understand the tongue of his parents. (255)

A third group comprised those (necessarily adolescents or adults) whose guardian spirit experience had given them the power to interpret the language of the spirits (239). Here there may have been not a single speech community, but individuation into a variety of dyadic relationships between particular persons and spirits; and membership may have overlapped that of the second community just described. The example given is that: "For instance, one who had gained the protection of Coyote could tell, on hearing a coyote's howl, what person was going to die" (239). The example suggests that the capacity of this code (or these codes) was low, sufficient to identify a name or event, but not normally expected to serve extended discourse.

The first speech community was normally partly multilingual, and, insofar as defined purely in terms of a common code, extended sporadically down the Columbia for some distance. As were others in the area, the Wishram were largely exoga-



mous. Families of prestige especially obtained wives from other groups downriver, and married their own daughters into such groups. These groups were largely speakers of somewhat different Upper Chinook dialects, partly speakers of mutually unintelligible Sahaptin dialects. The importance, fame, and pride of the Wishram as a center of trade presumably entailed bilingualism in dealing with the various Sahaptin and Salishan speakers who came to them and to the Wasco each summer and fall. The ethnography unfortunately says nothing of the communicative means employed in trade. With whites in the nineteenth century, it was primarily Chinook jargon; in aboriginal times, trade and trading partnerships among Indians, together with marriage and other intertribal relationships, probably entailed a certain number of Chinookan-Sahaptin, Chinookan-Salishan, Chinookan-Sahaptin-Salishan, etc., polyglots. There is no information as to what the Wishram thought the boundaries of the second and third speech communities to be; the language of babies and the language of spirits may have been conceived as coextensive with Wishram society itself, or not. In any case, the society provided for bilingual interpreters between these two specialized groups and Wishram society at large. The desirability of interpreting the language of spirits (as omen, etc.) is obvious. That old people might specialize in interpreting the language of infants is probably due to belief that babies might return to the land from which their souls had come, if not happy. Given the high value placed upon children, and the love for them, concern to interpret infants' cries is understandable.

The first Wishram speech community is our main concern, but the other two impinge. The varied and partial data as to the place of language will be made use of as follows. First, various pieces of evidence will be adduced that indicate a serious and distinctive Wishram valuation of the discursive use of language. Second, a pattern that constitutes certain Wishram speech events as formal discursive uses of language will be described. Third, three speech events — disclosure of one's guardian spirit vision, conferring of personal name, narration of myth — will be treated in detail. They will be analyzed in terms of the valuation and the pattern just mentioned, and in terms of dimensions which place them in a hierarchical relationship with respect to each other. These three events were not the only major ones in Wishram society, but there is reason to think them representative. We know them to have been of central importance to the society, and the ethnographic data about them is relatively good. Of most other events recorded in the ethnographic monograph and involving speech — prayer, first salmon ceremony, ear piercing, feasts of rejoicing over a child's accomplishments, succession to chieftainship, burial, marriage, etc. — little can be told or inferred as to the matters that concern us. Those other events whose records can be used are introduced into the next portion of the analysis.

*Valuation of discursive use of language.* There seems a clear distinction of attitude as between 'casual' and 'non-casual' uses of language. Indeed, for the Wishram, it might be said that the true distinction is between two types of non-casual use, the difference being that one is marked by the structure of participation in the situation, and features of style, as formal.

As an indication of the seriousness of discursive use of language, notice that the Wishram are exceptions to the putative universal of language use described by Malinowski as "phatic communion", and endorsed by Sapir as a general function: Wishram felt no need to fill silence with talk. Tolerance for long pauses was evident in my own work with linguistic informants. That talk was not mandatory is more dramatically clear by contrast to our own society in what might constitute a visit. A friend could come into one's house, sit, and leave, without a word being exchanged. One would later report, "So-and-so came to see me yesterday." That he had taken the trouble to come was communication enough. A visit need not include talk, if nothing needed saying.

The seriousness of discursive use of language was conditioned by an attitude which may be called 'perfective'. The attitude first came to my attention in connection with field work. The system of initial prefixes which most distinguishes Wishram-Wasco verbs from the verbs of other Chinookan dialects is not only a tense system, as it has been described in terms of the usual English translations, but an aspectual system as well. It now seems that the perfective vs. imperfective contrast is of central importance to it. This importance emerged when I tried to elicit future forms. There are two futures, one near, one remote, and the formation of the remote is an important, unpredictable mark of subclasses. My Wishram colleague, Philip Kahclamat, however, would supply the future form of a verb only when the event described could be considered certain to occur. Only when I could construct a hypothetical situation of certainty would he consider it proper to use the word. (A Wishram verb word can be a sentence, marking subject, direct and indirect object, and various aspectual, temporal and other relationships.) Further indications of this 'perfective' attitude of concern with the certainty of consequences attendant on speech will be found in what follows.

An attitude of seriousness, and an expectation of consequences from serious use of language, are indicated in evidence as to chiefs, councils, and adultery. As to chiefs, their word was implicitly obeyed ... If there was trouble within the tribe, it was the function of the chief to DECLARE what should be done. Whatever the decision, it must be obeyed (212, emphasis supplied).

The handling of death due to witchcraft by the chief's council is of special interest, because it excluded use of language on the part of the direct participants:

CONFESSION would not mitigate the penalty, and no other PLEAS were effective ... Inasmuch as the offender and his partisans were excluded from the DISCUSSION, there could be no effective ARGUMENT on these counts (counts of justification and accident) ... Cases always rested on circumstantial evidence and public knowledge. An eye witness never TESTIFIED, for his life was in danger if he did (from the defendant's kinsmen) (214, speech acts are underlined).

According to Spier and Sapir, three considerations affected the penalty imposed by the council: the evidence (circumstantial, plus any public knowledge); the relative ranks of murderer and murdered; a settlement satisfactory to all the principals in

order that the matter should rest with this solution. One wonders if direct use of language on the part of participants to the crime was excluded so as not to hamper the considerations important to the council. Possibly direct verbal evidence would constrain a decision, because of the inherent seriousness and imputed validity of language used in such a context; what was wanted was not so much the truth, as social peace.

An adulterer, if once caught and spared, was warned that if he was again found talking to the wife in question, the husband would be at liberty to kill him: "It mattered not what they talked about; they were not to talk together out of earshot of other people" (216-7). In other words, talk under such circumstances was constructive adultery. Because of the past relationship, such talk could NOT be casual talk, but must be taken as talk having consequences.

A clear case of the other side of the coin is found in connection with the chinook salmon (the first salmon of the year):

No one could talk casually and carelessly about it. Boys were told: "You must not say 'I am going to catch the spring salmon, to kill him.' He was a person. If you say that, you might be drowned." Boys who disregarded this and said they were going to catch many, always met with bad luck; they might be drowned, or at least would catch only a few (249).

In other words, such talk could only be construed as improperly casual talk, because the circumstances for fulfillment could not be guaranteed; indeed, under the circumstances, such talk jeopardized fulfillment and guaranteed a negative outcome. Such a remark simultaneously violated norms of cultural belief, talk, and grammar (recall discussion above of the perfective character of the future in Wishram).

*Pattern of formal discursive use.* The patterning of formal discursive use of language is clearest in situations that may be considered ceremonial. These situations were marked by a style of language and by two constituent roles of particular importance here, those of public, and of spokesman, or repeater. The term 'public' indicates that the audience was understood to constitute a formal part of the proceedings; its presence, and its being formally addressed, were integral to the event. Equally integral to the structure of the event was a person who addressed the public, by repeating the discourse of another.

Regarding the formal style, French has observed (1958: 263):

If a culture is taken to be a moral order, and if some events are more important than others, it is possible to understand certain forms of non-casual language as marking such events. Religious ceremonies (excluding shamanistic curing sessions), life cycle ceremonies, 'tribal celebrations', other meetings at which the public is assembled, and formal myth narration all share a moral quality and display one or another type of non-casual language.

Regarding the structure of ceremonial situations, French summarizes:

In the ceremonial situation in which the public is present, an ideal pattern is that neither the sponsoring family, nor any person who is the focus of attention in that family, communicates

directly with the public; communications are mediated through a spokesman and through non-casual language...

The pattern of repetition is noted explicitly by Spier and Sapir with regard to chiefs and shamans. They write:

Chiefs were provided with spokesmen (doubtfully, one for each), who repeated to the gathering in a loud voice what their principals said. This type of repetition is called *kixwau'ululix*. The spokesman might be any man; it is not clear that there was any specialization of function here. It is well to note that this is a pattern of Wishram procedure; a shaman also had his spokesman who repeated aloud what the spirit communicated to the shaman. The characteristic functionary of Northwest Coast chiefs will be recognized here. (213)

Of the chief in relation to publics, French (ibid.) goes on to state more generally:

The question was raised earlier as to when the chiefs utilized a spokesman to repeat their words and when they spoke directly. In the analysis above, the public was present when a status was being changed. It can now be suggested that publics were also constituted when statuses were MAINTAINED (sic) ceremonially. The chief needed no spokesman when he was engaged in routine business, e.g., vilifying White fur traders, even though he might employ a kind of non-casual language. When he was in the ritual state of being the chief vis-à-vis the people, a spokesman was appropriate.

Notice that French himself emphasizes the maintenance, or validation, of status as a feature of events constituted by public and spokesman. I think that the notion of such events as validation links the structure of such events to the 'perfective' valuation of language. I take such events as to have been considered by the Wishram as ensuring the consequences discursively verbalized in them. Exchanges of gifts (and, in the case of marriage, visits) were also essential features of those events concerned with relations between kin and tribal groups, as well as those concerned with change and maintenance of status (and those in which both were of concern). The converging of these three things, discursive formal language, structure of spokesman and public, and exchange, manifested the highest level of validation that it was within the power of Wishram society to bestow. Discursive use of language, and less obvious situations of spokesman and public, however, might also constitute validation, as I shall now try to show. Before taking up the three speech events of principal concern, let me indicate the presence of the combination just defined in other aspects of Wishram culture.

If a man died after an illness caused by witchcraft,

As he expired he repeated the very words used by the shaman in defining the manner of his death and stated who planned the deed (215);

As he expired he spoke the very words used by the shaman in sending his spirit on its mission, in which the shaman instructed it just how the deed was to be accomplished. The dying man named his rival as the instigator. (247)

As we shall see, this practice parallels the disclosure of one's guardian spirit vision at death. Here the victim is made the involuntary 'speaker' for the shaman's instructions at the moment when his dying validates their efficacy.

On war parties, men might dream, prefiguring the conflict. In the morning the chief and others would tell what they had dreamt; in the case described (229-30, 250-51), the dream was fulfilled. Spier and Sapir remark:

It is possible that these were derived from the guardian spirit, but it was not so stated. That is, there is not much difference between a dream in which a hunter was told by his spirit, the deer, where deer were to be found, and the following" (250; the war party example follows).

In short, the one telling the dream in the morning was a repeater of what had been conveyed to him, perhaps by his guardian spirit, at night; the others of the party were the public; and the prefigurement was expected to come true.

A notable instance of such dreaming, found among the Wishram, Kathlamet, and other Amerindian groups, was that of a man who told the people, before the whites had come, of what they would be like and what they would bring.

These two types of event show the prevalence of the pattern in question among the Wishram; they themselves, however, are hardly specific to the Wishram. I now turn to three events, also not specific as types to the Wishram, which have among the Wishram aspects not yet described, so far as I know, for any other group. One aspect is the hierarchical relationship among them. A second aspect, and the crucial one, is the way in each expresses the 'perfective' attitude toward discursive use of language, by showing such use as RESTRICTED. In each case, disclosure in the event is correlative with concealment outside it.

The three events are disclosure of guardian-spirit quest experience, conferral of personal name, and narration of myth. Each event will be discussed separately, in the contexts of which it is part. Each discussion will begin with the available ethnographic data, as to concealment and disclosure. The bulk of each discussion will then be organized in terms of dimensions found significant for comparison. Analysis and additional data will be introduced, insofar as needed to bring out the salient features on each dimension.

The salient features of each event are taken as those having to do with (1) its constitution as a formal event; (2) its scheduling; what might be termed its cycle, with respect to the timing of (3) acquisition, (4) disclosure, and (5) concealment of what is disclosed in the event; and (6) the scope of the event, with respect to (a) participation in it, (b) relevance, and (c) subsequent import.

Following the separate discussions, the three events will be compared on these dimensions; and the relationship among the events, and between them and the 'perfective' attitude, will be further shown.

*Guardian-spirit quest.* Data are presented by Spier and Sapir (236-248), with occasional cross-references in other sections of the monograph. The main facts concerning disclosure and concealment are given in the following part of the account:

The very secrecy maintained about one's spirit experiences offered opportunity to keep that fact (possible failure in the quest) concealed until occasion should arise when some success was achieved to hint at the possession of power. It does not seem clear that the lad who

returned home from his quest admitted anything more than that he had an experience, without revealing its content. Again, at spirit dances those with power hinted at their spirits by their actions, but no more. The full revelation came only at the point of death when all the details were recited. Under such conditions the stage was set to assume a very close causal relationship between spirit power and material success.

The revelation at death or in dire need had a form quite stereotyped. The dying man called for some article connected with his spirit, told how he came by his power, and recited that as an omen it would storm. (238)

Further data shows a distinction between revealing the identity of the spirit and revealing the content of the experience. Certain individual tabus might reveal the nature of the spirit guardian (238), e.g. refraining from deer meat would indicate a deer spirit. During the midwinter sacred period of spirit dances (December to March), a person might refer to his spirit in his song, by wearing a symbol, by imitative actions in the dance, and, sometimes, by appropriate costume, e.g., hat and belt of wolf skin for a wolf spirit (242).

Since a person would never speak of his spirit until his death, the only way in which the people generally knew of it was at such performances. (242)

The Wishram reticence was selective, rather than an automatic part of the spirit-vision complex in North America. The reticence is paralleled among the Ojibwa of the Eastern Woodlands, but within the west, it is strikingly dissimilar to the attitude of the Klamath (Spier and Sapir, 238). Spier and Sapir suggest that the Klamath follow a California tradition, and that the Wishram conform to a more general American view, together with the Ojibwa (pp. 238-9). Within the same culture area as the Wishram, however, the Sanpoil and Nespelem, Salishan speaking tribes to their northeast (between them and the Ojibwa) parallel the Klamath, not the Wishram. The distributional facts, sparse as they presently are, indicate that the attitude in question is culturally specific, not areal.

The pattern provided an opportunity for rationalized integration of one's life, suggesting the analysis of mid-life identity crisis given by Erik Erikson in his *Young Man Luther* (1958). Here a way to meet the need would seem to be institutionalized. One need not assume conscious distortion on the dying man's part, but only a certain openness to interpretation of the initial vision experience, and the selective remembering and rationalization common to us all. (Spier and Sapir's suggestion of conscious fraud as to success in initially obtaining a vision seems most unlikely; what is unlikely is that any youngster, honed keen with expectation — the metaphor of "sharpening" is a Wishram one — would encounter or experience nothing whatever that could be interpreted within the Wishram scheme of things as evidence of a guardian spirit. Almost any living thing, and some things that were not living, might be such a spirit.)

The salient features of the guardian-spirit quest, for comparison with personal naming, and myth narration, are these:

- (1) The event comprising disclosure by discursive use of language was constituted

by the individual concerned, as repeater of what a spirit power had previously said, and an ad hoc audience, as public. (In the vision a power spoke like a human, 240, not in a special code). "The revelation at death or in dire need had a form quite stereotyped" (238).

(2) The occurrence of the event was (in the nature of things) unplanned. Its frequency of occurrence as a type depended upon the death-rate of those with guardian spirits. Probably it occurred once or more each year.

(3) The identity of the spirit, and the content of its message — the power obtained, and the terms — were acquired together, by an individual alone at night. Youth was the only time at which the guardianship of a spirit or individual protector could be obtained. In effect, there was in native theory a critical period. After a certain period the power to acquire guardianship vanished (239). David French, in a personal communication, observes that among Sahaptins, and possibly Chinookans, spirit power could be gained by adults, and shamans could use existing powers to get another (the patient's) during a curing ceremony. Melville Jacobs posits adult acquisition among the Clackamas Chinook. If adult acquisition did occur among the Wishram, presumably it was by way of increment to power already obtained, adding new power or intensifying an already existing relationship. 'Entering into relationship', indeed, is the better term for the basic experience; subsequent experiences, including dreams and visions during 'dying' (loss of consciousness), added instruments and information.

(4a) The identity of the spirit relationship was disclosed publicly to all who participated in the winter spirit dances, soon after the individual entered into the relationship (240-1). Disclosure was by non-verbal means and in song. That the song disclosed only identity, not content, is indicated by the brevity of such songs, and by analysis of the verb used. The idiomatized theme, *-xn-gi-xnuma-kw*, is glossed by Philip Kahclamat as "to sing a tune with words about one's power". The stem, *-xnuma*, is not now analyzable, although the *-ma* may be causative (the shape of *-xnu* suggests perhaps *-xul* "to talk", *-xliu* "name", and *-qanu-čk* "myth", the *l/n* and *q/x* alternations being possible.) The affixal material specializes the action as reflexive, in relation to oneself alone (*-xn*), as without direct object (*-gi*), and as going on round and about (*-kw*).

The song might be joined in by others, or repeated by one or more others (241, 242-3), thus constituting the act of disclosure itself as a formal event, validating the identification.

(4b) The content of the experience was disclosed at the dying, or presumed dying, of the individual. (For an instance of the latter, see 245.)

(5a) After the spirit dance, one's identification with a spirit power was not concealed. It was public knowledge.

(5b) The content of the vision experience was concealed until the time of dying. From the standpoint of the society, the nature of the experience might have been taken as reflected beforehand in the individual's activities and degree of success.

From the individual's standpoint, the content of the relationship might be amplified by ad hoc experiences (dreams, illnesses, adventures, etc.).

(6a) Participation in the act of disclosure was ad hoc; there might be no others present at all. (I believe a man dying alone would, if possible, sing his song and address himself to his power and the powers of the environment.)

(6b) What was disclosed was primarily individual in reference. The specific content was wholly so. A child was rigidly instructed and guided in obtaining a power (257), because of its importance to success in life, but the guidance was in procedure only. "The child knew nothing of what he was to expect, nor did the one who sent him on the quest" (240). Consider also that the term for preparing, training, for spirit experience is reflexive, meaning literally (in the future inflection given in the text, 239), *a-l-x-la-y(a)*, "someone (*l*) will (*a*- ... -*ya*) move (*la*) himself (*x*)". (As alluded to above, the stem, when not reflexive, is used in the meaning "to sharpen".)

Power was sought, but the act of acquisition was itself looked upon as more or less involuntary. The spirits from whom power was sought were many and various, comprising most the inhabitants of the natural world (236) and rocks and trees as well (237); and there seems to have been a plurality of spirits for each form, e.g., deer, sturgeon. The accounts speak of *A*, not *THE*, deer spirit; of sturgeon *SPIRITS*; etc. Thus, what was acquired was individualized, as to the particular spirit with whom one established relationship, as well as with regard to the content of the particular experience.

Notice that these facts not only enhance the uniqueness of the experience, but also reinforce the public ignorance of its content.

(6c) Once disclosed, a vision experience might be repeated by others, perhaps as news or gossip, but sometimes later as a tale, or even eventually as ingredient of a myth. Of three brief instances of disclosures given by Spier and Sapir (240, 245), one is from a tale, another from a war narrative. Boas' Kathlamet informant, Charles Cultee, told with pride of his grandfather's guardian spirit experience in nearly legendary form, and there are Clackamas myths centered on guardian-spirit experiences.

Before disclosure, the identity of a spirit might be of general interest. Recall the ability to interpret the vocalizations of infants given to old people with certain spirits. To be a war leader, one almost always had to have a spirit such as a rock, tree, or sturgeon, which kept one from being wounded, or, if wounded, from bleeding to death (237). A shaman could not cure a bewitched person, if his own spirit were not more powerful than the spirit intrusive in the patient (237, 245). Prior to disclosure of the content of the experience, also, participation in the annual midwinter spirit dances helped maintain both an individual's relationship with a spirit, and the society's relationship with the world of supernatural powers generally. The singing of one's song seemed to have been a focal point in this regard; participants would sing their song once each night (241).

As with the content of the experience, so with the manifestation of relationship through song: *the significance was primarily for the duration of one adult life*. Power



and song could not be transmitted, only the legend and knowledge of their existence. Nor did the Wishram fight much, except in retaliation, stopping as soon as one or two were hurt; and too powerful a shaman was dangerous and likely to be killed. Still, such facts show that the vision experiences of at least some individuals were of interest and significance to others, during their own lifetimes, occasionally beyond.

*Personal name.* Data are presented by Spier and Sapir on pp. 258-260. The main facts concerning disclosure and concealment are as follows. First, regarding disclosure:

Without doubt the greatest event in a Wishram's life, and certainly the most impressive ceremonial of the ordinary individual, was the bestowal of his name. A child was first named when he was from six or eight months to two years old; he might receive a substitute when adolescent, and perhaps again as an adult, during a shamanistic dance. Occasion arose to change the name again on the death of a relative, when there was less ceremony or perhaps none at all...

The naming ceremony was quite impressive. A feast was prepared and relatives and friends even of other tribes were invited. Very valuable gifts were made to the spectators, so that the person would be widely known by this name. The one who was to be named was dressed in a costly costume and stood between two others in the midst of the gathering. The one on his left began in a low voice: 'This is now so and so.' [For realism, I hereafter substitute an actual name, *támxat*.] The one on his right repeated this in a loud voice, the people answering loudly: "*a:xi:*". The name given was that of some relative who died long before; probably some of the younger people had never known or even heard of this person. Then again the one on the left said in a low tone: "This name used to be *támxat* who died long ago." The one on the right repeated these words in a loud voice and again the spectators answered: "*a:xi:*". Now the one on the left said in a low tone: "We want the mountains, the rivers, the creeks, the bluffs, the timber to know that this man is now named *támxat*." He on the right repeated this in a very loud voice, the spectators responding: "*a:xi:*". The left said again: "We want to let the fishes, the birds, the winds, snow and rain, the sun, moon, and stars know that *támxat* has come to be as though alive again. His name will be heard again when this man is called." The right repeated as usual, the people answering: "*a:xi:*". This was the last announcement. Anyone among those present who was ever acquainted with the former bearer of the name could now come forward and ask for whatever he wished. He said: "I am glad to meet *támxat* again after being so long lost (dead). I am glad this name has come to be spoken again; so I want *támxat* to give me..." It was invariably given him (258-9).

Spier and Sapir remark: "The importance attached to names, which were almost titles, and the desire to have them widely known is distinctly reminiscent of the Northwest Coast proper" (259-60). This title-like quality of the names is brought out in the data as to acquisition of a name later in life. The accounts imply validation of a valuable property. Thus:

For instance, *Táxšani* died; after five years his son would call all the people of the neighborhood to a big feast, giving away many presents, and paying an important man well for then calling him by his father's name. (258)

The third, composite description given by Spier and Sapir seems also to be that of conferral of a substitute name later in life:

... An old man would rise and address the gathering. He repeatedly lifted a nicely tanned elkskin and pronounced the name. "You used to call him by that name when you met him

on the road. Today he is going to leave that name. Today he will be called by his grandfather's name, *čápalai*. You will know it when you meet him, and you will call him by that name." ... Another man rose: "All you who are gathered here in this house! A long time ago there was *čápalai*: he is going to be with us again. We will give this name to the child." ... Another rose: "He was going to give me a horse, that name." The parents would say: "Yes, we give you that horse; he is going to be with us again." ... (259; again, I have substituted an actual name.)

#### Regarding concealment:

There was a marked objection to telling names, one's own or another's. ... The observances with respect to names were strictly adhered to. (258)

The situation with regard to disclosure and concealment, use and non-use, of personal names becomes clearer when practices concerned with the death of the bearer are considered.

A name was always that of a dead elderly relative; there was no exception to this. ... When a man died his name was not uttered again for five years. Some close relative was then given it, a son or grandson (258; the case of *Táxšani*, quoted above, is an instance.)

The interdiction on use of the name between the death of one bearer and resumption of the name by a descendant was associated with a special expression, used by older people to correct a child who mentioned the dead person's name. Philip Kahclamet gave as example a child saying "Paum" (name of Norman Wolfe, now dead), and a parent saying, "*k'ánaniš, ilkašušu aga*". (The first word, *k'ánaniš*, expresses the prohibition, and is unanalyzable; the second word expresses the fact of death, together with *aga* "now"; its stem, *-kašušu*, seems euphemistic substitute, perhaps with reference to a hushed voice, for the normal form, *ilmimlušt*. The forms are from the field notes of David French.)

The nearest thing to tabu affecting other words concerns, not words associated in shape with the name of the deceased, but other names, associated with the use of the name of the deceased in social relationships:

The name might be changed at any time in later life when a relative died. (There was less ceremony or perhaps none at all.) This was done out of deference to the feelings of the parent of the one whose name was changed; he might feel sad to hear his child called by the name the dead person had used for him. McGuff [Sapir's interpreter] specified that the change was made when a parent or brother or sister died. (It is significant that the change was not made on the death of one's own child, implying that children never called their parents by name.) Sometimes all the names of a family were changed in this fashion. Members of the family did not like to use a name which had been used by the deceased. That would be looked on as mocking the dead. (258).

Since, as Spier and Sapir say, there was less or no ceremony, it is very likely that the changes were restricted in scope to the family, and that outside it the previously, prominently acquired names were kept in currency (see (2c) below). Indeed, according to native theory, those outside the family could not know the names privately acquired under the circumstances just described.

The rules of use and non-use can now be stated more generally and analytically.

Use of a name in a context of reference was proscribed outside the ceremony of conferral. Use of a name in contexts of address was mandatory in the ceremony of conferral; common within a family from parent to child and between siblings (as the practice of changing implies); and permissible, perhaps sometimes mandatory, more generally (as the reference in the third ceremony described above to "when you met him on the road" suggests). Texts indicate that kin terms were the common form of address, both to relatives and others. Use of a name in reference or address was proscribed after the bearer's death, and the proscription attached not just to the name of the deceased, but to a set of names: the set whose use had implicated the deceased person both as addressee AND AS ADDRESSOR within the family. (This usage freshly supports the inference of the prominence of a bipolar orientation toward experience among the Wishram, made in an earlier paper relating Whorfian concerns to Chinookan data; cf. Hymes 1961a, sec. 5.)

The salient features of the personal name, for comparison with the guardian-spirit quest, and myth narration, are these:

(1) The event comprising disclosure by discursive use of language was constituted by the family concerned, as sponsors; an invited audience, as public; and two spokesmen for the sponsors, one repeating to the assembled public the words of the other. (The presence of TWO spokesmen, unique to this event, helps stamp it as "the most impressive ceremonial of the ordinary individual". Notice also that others participate in speaking as well.) The accounts indicate conventionalized forms of expression were used in a conventionally structured sequence (see 4b below).

(2) The occurrence of the naming ceremony was scheduled by invitation. Its occurrence was regular with regard to the infancy of every child surviving the first few months of life, and perhaps regular with regard to some occasions for some individuals in later life (e.g., perhaps with regard to succession to chieftainship, 6b). Frequency of occurrence of the ceremony as a type depended upon the general birth-rate, and perhaps upon the death-rate of any whose names a successor would usually want, or be required, to adopt.

(3a) At least one name was acquired by every Wishram, early in childhood; some acquired one or more names in addition. Apart from change of name within a family on death of a member, the acquisition of additional names probably correlated with wealth and social rank. From the standpoint of the society, a name was conferred once in each of a series of consecutive lives.

(3b) The content of a name was conferred simultaneously with its identity, from the standpoint of the individual; in the light of the analysis that has been given, it is possible to suppose that the conferral was something of a claim or promissory note. As a title, the name carried implications of achievement which could not yet have been realized in the life of the recipient at the time of acquisition. In this regard, notice the contrast between the conferral of names in the naming ceremony and such conferral in myths. In myths, specific attributes of condition and behavior are pronounced. In the accounts we have of naming ceremonies, no specification is made. I take this

difference to depend on the perfective valuation of discursive use of language. Within the mythical framework, it was within the power of Coyote or another actor to accomplish by pronouncing what was pronounced; and the subsequent experience of all Wishram testified that the accomplishment was valid. Swallows were indeed called "Swallow", were birds, and came at the time of the first salmon run in spring. The spokesmen at a naming ceremony had no such power. Their words could validate the identification between the person and the name, but not the subsequent fate of either. The conduct of the ceremony could validate the prestige attached to the name in relation to its last bearer, by gifts and expressions of gladness at re-instatement. As has been said, the very choice of the name by a descendant as worthy of the major ceremony of an individual's life and of proclamation to kith, kin, and the natural world, was validation of that sort. But again, the ceremony could validate only retrospectively, not prospectively, so far as prestige-content was concerned.

I see here a basis for understanding the subsequent use of a name in address, but not in reference. Use in address presumed only the validity of the identification of the name with the individual; use in reference might be taken to imply pronouncement of the attributes as well. In reference, one would have to use one of the expressions found in the final scenes of myths, e.g., either *támxat i-ya-xliu* "Tamxat (is) his-name", or *támxat i-u-pgna-t* "he is called (pronounced) Tamxat". In address, neither the noun nor the verb would occur. In effect, in address one was quoting, quoting from the naming ceremony as to the identity of the person addressed. (Viewed in this light, the use of names in address is parallel to the use of elements of myths outside the context of formal myth narration.)

(4a) The identification of the name as that of the recipient was disclosed publicly, verbally, in the act of acquisition.

(4b) The content of the name was disclosed implicitly during the ceremony, by explicit reference to the past bearer. SUCH REFERENCE COULD NOT BE MADE AT ANY OTHER TIME. (As to why the disclosure was verbally implicit, see discussion in 3b and 6b). Presumably the prestige-content of a name, as title, was cumulative and selective over time, through the lives of persons bearing the name, and the image of such lives held by contemporaries and descendants. (The nature of the part played by verbal discussion is not known.)

Notice that the ceremony manifestly dwells on the past bearer, by proportion and cumulation of emphasis. There are four announcements, successively more complex, two stating the identification, and two addressing the natural world. In each pair, the first announcement refers to the new bearer, and the second then refers to the former bearer. After the announcements, the persons coming forward speak, not in terms of the new bearer, but of the old. In other words, the order does not start with reference to the past, then look forward to the future. It starts with the present, and then looks to the past. In this structure, ending on rejoicing directed to the PAST identification of the name with a person, I see further evidence that the ceremony validated prestige-content retrospectively.

(5a) After the naming ceremony, identification with a name was not concealed. It was public knowledge. In native theory, however, persons not present at the ceremony could learn of the identification only from contexts of address. Possibly some situations required use of the name in address — perhaps, e.g., the investiture of a chief — but I have no data on this. From the standpoint of the society, the general relationship of identification between members and title-like names of prestige in the eyes of the world must have been maintained by one or more ceremonies annually.

(5b) The content of the name can be taken as concealed, so far as discursive use of language is concerned, apart from the ceremony of conferral, where reference to possession of the name by a bearer was explicitly made (see 4b). From the standpoint of the society, the nature of the content could have been taken as reflected subsequently in association with the individual's activities and reputation. From the individual's standpoint, the content of the name might have been amplified by subsequent experiences, but again, data is lacking.

(6a) Participation in the act of disclosure was by invitation to a large body of people, kith, kin, and friends from other groups (258).

(6b) What was disclosed was both individual and social in reference. On the one hand,

No two people within the tribe bore the same name, although it might occur among another Upper Chinookan people. (258)

Also,

Names did not have meaning ... (and) reading meanings into them ... is certainly not the habit today. (258)

Subsequent field work confirms the etymological opaqueness of the names, and the absence of folk etymologizing. At any given time, then, a name could identify no one among the Wishram other than one individual.

On the other hand, the conferring of the name involved explicit appeal to the underlying relationship between the social and natural components of the Wishram world as a whole. The name itself had a title-like character, and a certain socio-cultural content (or set of connotations). Names were specialized for sex, and, as we have seen, one name implicated others in a set used reciprocally in address within a family. More importantly, a name was necessarily obtained by transmission along kinship lines. It was a social property, maintained in the group, and this continuity was explicitly stressed in the ceremony of conferral. In Wishram social theory, the set of names endured, and particular lives passed through them.

Notice that the fact that names had no descriptive, etymologizable meaning and that meanings were not read into them, had as consequence that the content or value of a name could accrue to it ONLY through its identification with one or more of a succession of bearers. The name itself named no attributes, but only by implication the attributes of those with whom it had been associated.

The title-like function of personal names must be seen against the background of a fabric of kin terms. Kin terms provided means by which every member of the community could be appropriately referred to or addressed, either as an actual relative, or by appropriate extension (e.g., "grandfather" respectfully to older men). The fundamental social identity of a person was already handled by his or her place in the kinship matrix. Personal names had a superadded function.

The title-like function, and association with content, become clearer, if we consider Wishram naming in general, and the two pertinent Wishram forms, the noun-stem *-qliu* (possessed form, *-xliu*) "name", and the verb-stem *-pgna* (sometimes in WT, *-pquna*), "to name".

First, regarding Wishram naming: David French has shown that Wishrams (and Wascos) contrast with English speakers AND other Indians in the area, when confronted with unfamiliar objects (plant specimens). Failing fit to a specific existing category, English speakers tend to go up a level in a taxonomic hierarchy, and Sahaptins analogize; but Wishrams refuse any name. This behavior suggests a very fixed sense of a name as implying specific attributes, qualities, conditions (analogous to the perfective use of verbs).

Such an attitude fits the two Wishram forms. Both indicate more than a label and its application, more than act of reference. They have a constitutive force often, a degree of inseparability from the specific features of what is named, especially where persons (human or supernatural) are concerned.

With regard to *-xliu*, a textual example occurs when Sapir's informant discloses that he has not had a guardian-spirit experience. He states (literally): "But not I have seen thing its-name guardian spirit." Now he has just described what was done "to prepare one for a guardian spirit" without *ia-xliu* "its-name". In its context, the use of "its-name" emphasizes: I have seen things, but not something whose characteristics would entitle it to be called, addressed as, a guardian spirit (WT 190: 9-11).

Intimate association between "name" and attribute is recurrent at the close of myths. In many myths the final scene is one in which one or more myth persons receive the identity they are to have in the ensuing period (eternal, in native conception), when the Indians shall come. In such scenes, *-xliu* is an indispensable element. Thus:

"This day (she is) black (and) Crow (is) her name ... this day her name (is) Bald Eagle (and) white (is) her head" (WT 98: 14-16);

"You two," he said to them, "for all time your name (shall be) Raven; thus shall you be" (WT 65: 22-23);

"By what right, perchance, would you, Owl, do thus to people? No! This day your name (has become) Owl." (WT 38-6-8);

"He got some dust, threw it at them, and said to them: "You shall be no chief. You (are an) animal; your name shall be Antelope". And then they started to run away, all gray (WT 74: 11-14).

The act of naming and the fact of identity are inseparable. Together with some physi-

cal trait (usually of color), status as an animal, a certain kind of animal, is acquired forever: "For all time your name (shall be) Raven; thus shall you be".

With regard to *-pgna*, it must be observed that neither this nor any other word can be translated accurately as simply "to name". (Nor has *-pgna* any etymological connection with *-qliu* "name"; if it has any analysis, it is into *-pq-na*, not *-p-qna*). Most generally, *-pgna* has to do with the proper (or effective) USE of names; and the one or two other verb-stems have to do with MISUSE of names, respecting the dead. The range of meaning of the stem can be indicated as "to call by name, to call out, to address, to pronounce". The active transitive construction in the present is taken as "to call out", e.g., *č-i-u-pgna-t* "he (č) has (-t) him (i) called out" (all examples of this use are so taken, as certain of accomplishment). The intransitive construction is used specifically to designate the condition of a person in terms of legal or social status: *i-u-pgna-t a-ya-k'auk'au* "he is (has been) pronounced a murderer"; *i-y-u-pgna-xit a-ya-k'auk'au* "he became called a murderer" (*-xit*, passive causative); *št-u-pgna-t i-šta-k'amlā* "those two are called, pronounced bad, guilty". Regarding the election of a chief, an informant is quoted as saying, "He is respected and called a chief" (211); the underlying Wishram forms must be: ...*i-u-pna-t i-štamx*. An even more striking example is found in Sapir's texts. With regard to guardian spirit training, there is said to be an inspector, appointed to look after the work of those who are training. The person is *l-u-pgna-t* (WT 187: 24). The translation "appointed" shows the strong constitutive element in the word in ordinary life.

In the final scenes of myths, the form may have its force made even dramatically explicit. Salmon were the Wishram staff of life, and the first myth of the Coyote cycle tells how he set matters right in this regard, outwitting and transforming the two women who had hoarded the salmon in the mythical period.

He (Coyote) said to them: "Now by what right, perchance, would you two keep the fish to yourselves? You two (are) birds, and I shall tell you something. Soon now people will come into this land. Listen!" And the people could be heard "dulululu" (like thunder rumbling afar). "Now they will come into this land; those fish will be the people's food. Whenever a fish will be caught, you two will come. Your name (*i-mda-xliu*) has become Swallow. Now this day I have done with you; thus I address you (*ya-md-u-pquna* 'I (covert) pronounce it (*ya*) to you (*md*)'), 'Swallow'. When the people will come, they will catch fish. Then you two will come, and someone will address you (*a-q-md-u-pquna-ya*), 'The swallows have come; Coyote called them so (*ga-č-š-u-pgna*)'. Thus will the people say, "From these two did Coyote take away their fish preserved in a pond; now they have come." Thus did Coyote call those two (*ga-č-š-pgna*) (WT 6: 13-26).

The final two occurrences of *-pgna* practically imply the entire action of the scene — the giving of the proper name, Swallow; the establishment of the identity of the two myth persons as birds; the causing of the coming of the swallows to coincide with the first salmon-run in spring (what the people are to say alludes to this). Name, nature, behavior are bound up together in Coyote's pronouncement. (The significance of naming, pronouncement in myths in relation to that in the naming ceremony is brought out more fully in 4b below.)

The expression "today" that occurs in the naming ceremony quoted last at the outset of this section could only have been expressed in Wishram by the same phrase, *dauya wigwa* (literally, "this day") that runs through the concluding pronouncements in myths just cited.

It seems clear that a name, and to call by name, implied a content beyond mere referential ticketing. Names were not only sex-linked, and kin-linked, but prestige-linked as well. The express desire to have the name widely known and impressively validated, the most widely known and impressively validated of any attribute, testifies as much. In this respect, consider that some choice of name was possible, especially for a man acquiring a name later in life. In the two accounts of such a ceremony, one involves a father's name (258), the other a grandfather's (259); and notice that the description of the first entails a big feast, many presents, and paying an important man well for then calling one by the name (258).

From this point of view, the reinstatement of a name honored not only the last bearer, and the new incumbent, but also the kinship line as a whole, as having continued and as having the ability to provide a sufficient feast and gifts. Indeed, I think that the proper interpretation of the ceremony is that what is revived is not so much the dead person as the inactive title. The revival is a reinstatement rather than a reincarnation. The usages quoted by Spier and Sapir make sense from this standpoint. Where the name occurs, the title or status, rather than the individual, can well be understood; the other references to the deceased individual do not require his reincarnation to be assumed ("so and so has become as alive again", 259). Indeed, on this interpretation one need not query, but can accept as patently correct the statement in one ceremonial description which Spier and Sapir present as follows:

Another rose: "He was (is?) going to give me a horse, that name (man?). The parents would say; "Yes, we give you that horse; he is going to be with us again." (259)

The statement can be seen as not confused, but as making explicit that the continuity was of title (name), and that the parents' gifts were to validate identification of the title (not of the deceased bearer) with the child.

From this standpoint, the etymological meaninglessness can be considered significant. Spier and Sapir devote a paragraph to explaining how such meaninglessness might have come about in a historically intelligible way (258); they do not consider that the meaninglessness might be maintained in a socially functional way. One possibility is that specifically descriptive names (as are many bird and animal names) would circumscribe the range of descendants by whom it might appropriately be adopted. Given a limited set, there would be flexibility in having the content not overtly marked, but something that could accrue and evolve. An observation based on the names themselves has to do with their makeup and length. The names are all polysyllabic, and they frequently contain phonemic material that resembles known affixal material in the language, although it cannot be identified as such. (Such are occurrences of *k*, *q*, *k'*, *g*, *g*, *x*, *ɣ* plus *a*, *i*; *wa*, *wi*; *na*, *ni*; *ča*, *ca*, *la*; *-š*, *-t*.) Any



word or stem of such length would normally be analyzable into two or more meaningful elements. (Almost all Wishram affixes are monosyllabic, many monophonemic, and there are many monosyllabic stems.) Similar cases of polysyllabic forms not analyzable or translatable (or at least no longer translatable) occur in myths, especially in the song in myths. There may be here an appreciated, or even cultivated, connotation of hidden, archaic meaning.

(6c) Once acquired, a name was remembered as pertaining to a person, as indeed was the point of the ceremony. It is likely that acquisition of a second name did not wholly supersede a prior name. It has been suggested above that such was the case for change of names within a family after death of a member, others retaining the earlier name. Spier and Sapir identify three names as those of young men, three as of young women, three as of old men, and four as of old women (260). In one case (that of "Louis Simpson", Sapir's informant, cf. WT 224, n. 1), they identify two names for the same person (260): "pápkas, young (?); said to be that of a former Wishram chief. mǝnait, old; the common name of the same man in later life".

As this case suggests, names seem generally to have been remembered as having been those of chiefs (pp. 211, 212 cite K'alwaš, Slákiš, and a later Slákiš as chiefs). Indeed, there is some suggestion in the data that to be a chief, one had to adopt the name of a former chief, even if to do so meant transgressing the custom of adopting the name of an ancestor of one's own. Usually a chief (or headman) could acquire a chiefly name through the normal practice, since the position was hereditary. There were several chiefs. If the family line of one died out, the people would select a successor, who then married some other chief's daughter. Such an elected successor had no chiefly ancestral name; it would seem that he adopted one from the family of his wife (as in the case of the later Slákiš, 212). Such a practice strengthens the case for a prestige-content to personal names.

In general, a name's significance endured beyond the life of an individual bearing it. Any significance maintained, added, or lost to it in one life might endure as descendants necessarily chose from among the names inheritable by them.

*Myth narration.* Spier and Sapir do not much discuss the context of myth narration, but the general pattern among Chinookans and others in the area is well known, and details for the Wishram and Wasco are confirmed by later work.

The salient features for comparison with the guardian-spirit quest, and naming, are these:

(1) The event comprising disclosure by discursive use of language was constituted by an older person, as narrator, and an audience comprising especially children. The audience responded regularly to show its attentiveness. In one sense, the narration was a repetition of something learned from previous narrations, and in principle, in a chain extending to time immemorial. In a specific sense important to our present purpose, the narration was a repetition (in its crucial closing scene especially) of words spoken before by myth actors in the period before the world had taken its present settled form. Examples have been given in the section on naming just above

(2b). In narrating the myth in which Coyote releases the salmon, for example, one is to be taken as literally repeating the exact words spoken by Coyote, as he laid down that portion of the ways of the world.

Wishram were quite sensitive to correctness of repetition, and present-day informants who know a good deal of the mythology will refuse to narrate any, rather than narrate it wrongly. Wrongness here seems to extend to specific verbal form. The form of myths was quite highly conventionalized, both as to overall structure and as to detail.

(2) The occurrence of a particular narration might be scheduled by invitation to an esteemed narrator, from the standpoint of an individual household. As a type of event, myth narration was regularly scheduled each winter season by a household with children, and, from the standpoint of the society, it was recurrent throughout the season.

(3) A knowledge of many if not most myths was acquired by every Wishram from early childhood. Such knowledge included a command of the identificational aspects (see 6b), but presumably less commonly a command of the full content. From the standpoint of native theory, myths had been laid down in an immemorial past, and re-enacted annually since, as a joint community inheritance. In principle, all myths were recurrently accessible to all members of the society.

(4a) The identificational aspects of myths (see 6b) were disclosed in the narrations of winter nights, and might be employed year round, as occasion prompted or required. A hypothetical person not present at the formal narration of a myth might guess at the identification from observing its use in other contexts. Presumably children sometimes observed such use before having properly heard the formal narration. Some situations REQUIRED quotation from or allusion to myths, e.g., the coming of the salmon in spring. Myths could be referred to by titles or first lines.

(4b) The full content of myths, as re-enactment, was disclosed annually, but during winter nights only. Winter was in effect a sacred season, during which the spirit dances and myth narration characterized a society somewhat in retreat, drawing nearer to its supernatural sources of strength, and reaffirming its ties with them.

(5a) Identification of something as an aspect of a myth was not concealed; it was public knowledge.

(5b) The full content of myths was concealed in summer, through tabu on formal narration. It was believed such narration in summer would provoke natural sanctions. From the standpoint of the society, the nature of the content might be taken as reflected in the society's activities in the summer (see comparative discussion to follow).

(6a) Participation in the narration of myths was open to all members of the society, and required of children. If children slept during a narration, they were disciplined. In a given case, participation might be by invitation to the household where a narrator was to perform.

(6b) Myth was almost entirely social, or cultural, in reference, and was shared in all essentials by all members of the society. Individual narrators might not know or use

the same stock of stories, and might not narrate the same story in the same way (but each would believe his version correct — e.g., WT 9, n. 3).

The content of major myths disclosed the character, or nature (*-k'ani* — cf. WT 44:13, 48:1) of supernatural persons, upon whom the founding and the maintenance of the society depended (cf. Hymes 1958, 1959, 1965). The individual relationship to a guardian-spirit protector was writ large in the society's relationship to the natural-spiritual world as a whole; the relationship was of the sort Robert Redfield has termed "participant maintenance", and myth gave it ideological form. Myths also disclosed components of personality, as Melville Jacobs has shown extensively for the Clackamas Chinook. My own analysis stresses the cognitive aspect of this disclosure. Myths provided a calculus of motives, a way of abstracting, handling, and comprehending, through symbolic action, components of motivation not found in pure form in actual persons.

The content of myths recurrently identified and laid down particular attributes, including names, of many aspects of the natural environment, birds and other animals, plants, places. Features of myths also served to typify and label persons and personal traits, e.g., "I am hungry" (*walu gnuxt*) would recall Coyote, who frequently said it, and it might be jocularly used with that in mind.

Myths of course had the express function of inducting children into the common beliefs and values of the society. The Wishram were greatly given to verbal means in socialization, and myth narration was its most highly institutionalized form. The Wishram world was a settled annual round (*-lx* meant both "year, world"), laid down, it was thought, forever (*daminwa*). Myth showed the Wishram world in the added dimension of origins, weaving together familiar detail and dramatic action, the enacted past and the observed present, in a mutually supporting whole.

(6c) Myths served year round as sources of allusion and quotation (4a, 6b). In native theory, myths were an enduring possession of the society.

*Comparative analysis.* The salient features for comparison can be brought together in a summary table, Table V.

The first dimension summarized in the table has to do with those features which constitute the event a formal one, suitable for the discursive use of language in some act of maintenance or validation. As the table indicates, every event shows the pattern constitutive for the Wishram of such events: a speaker as source, a speaker as addressor who repeats the words of the source, and an audience. The full panoply is present together in the naming ceremony; the myth narration and the guardian spirit disclosure have been analyzed as manifesting the same underlying pattern: the narrator dramatically re-presents the words of Coyote or some other myth actor, and the visionary discloses what had been spoken by his tutelary. The naming ceremony is clearly an act of publication; it is the same with the other two events: what is disclosed by repetition would not otherwise be publicly knowable. In keeping with their nature, each event is characterized by non-casual use of language and by a high degree of conventionalization in the form of what is said.

TABLE V

	MYTHS	NAMES	SPIRITS
1. Constitution	Myth actors, narrator, audience.	Left speaker, right speaker, audience.	Spirit, visionary, (audience).
2. Scheduling	Regularly, each year.	Regularly, each life.	Unplanned
3. Acquisition:			
(a) by person	All often from childhood.	All once in childhood; some more after.	Many once in youth; some more after.
(b) of trait	Transmitted yearly.	Once each in series of lives.	Not transferrable.
4. Disclosure			
(a) Identification	Narration; in quotation.	Ceremony; in address after.	Spirit dance; dance after.
(b) Content	Winter only.	Ceremony only.	Dying only.
5. Concealment			
(a) Identification	None.	Reference, in life; from death to next conferral.	From experience to next dance.
(b) Content	From one winter to next.	From one conferral to next.	From experience to dying.
6. Scope			
(a) Participation	Available, all; required, children.	Kith, kin, friends, invited.	Ad hoc.
(b) Primary relevance	Community, and its world.	Status of person and of kin group.	Individual success.
(c) Subsequent import	Continuing possession of community.	Continuing possession of kin group.	Occasionally.

The other dimensions summarized in the table have something to do with the constitution of the events; all interrelate. Primarily, however, the other dimensions manifest the places of the events in relation to each other, and their relationship to the underlying 'perfective' valuation of discursive speech.

The events, and the institutions of which they are part, can be placed in a hierarchical order. The basis and nature of the order is shown most immediately by (6b),

the scope of the primary relevance of each. Myth is primarily social in reference, and has relevance to the community as a whole; the guardian spirit quest is primarily individual in reference, and has relevance restricted largely to individuals; the personal name is both social and individual in reference, being relevant to a group smaller than the whole community, but larger than the individual. Most of what has been said with regard to each event, and has been summarized in Table Five, falls into place in terms of the scale so defined. Myth, personal name, and guardian spirit quest range themselves in the same order with regard to frequency and regularity of scheduling; number of those acquiring, and frequency and regularity with which they do; frequency and range of disclosure of identification; correlatively, lesser degree of concealment of identification; range and basis of participation; scope of subsequent import.

This order can be seen as reflected also with regard to the perfective valuation of discursive speech. Each event presupposes this valuation (as will be discussed more fully below), but the formal disclosure can still be seen as admitting of degrees of certainty. That of myths is attested by many narrators, common knowledge of the myths, and the many incidental correspondences between myths, culture, and environment throughout the year. The certainty regarding a name is attested in the most impressive ceremony available to the society for constituting something a social fact, and in wide knowledge, but it is not supported outside the society itself (the natural world is addressed, but not implicated). The certainty regarding a guardian-spirit vision is attested by one individual *in extremis*, subject to some external confirmation as to effects, but not as to original content.

So far we have found a common pattern among the events, as formal events, and an ordering among them, as events of social import. What about the relative timing of disclosure and concealment? From the start, this has seemed crucial to understanding, but it does not enter much into the pattern and order so far found. Is there a common pattern? an order? or nothing, in this regard?

There might seem nothing. Concealment appears to precede disclosure in the case of the guardian-spirit vision, but follow it in the case of the naming ceremony. The winter-summer alternation of disclosure and concealment in the case of myths may appear simply irrelevant, a wide-spread trait arising from diffusion, and not particular to any Wishram pattern.

I do find a coherent pattern running through all three events, as to timing of disclosure and concealment. The showing of the pattern will depend upon two things: (1) the distinction that has been made between IDENTIFICATION and CONTENT (the terms are not happy ones, but the distinction is indispensable); and (2) the relation of disclosure and concealment to the 'perfective' valuation of disclosure.

The distinction has a recurrent, formal basis. It can be stated as a distinction between what can be QUOTED (song, name, myth remark), and what cannot. What can be quoted has to do with the identificational aspect or relationship; what can be disclosed only in the context of the event formally constituted for disclosure through discursive use of speech is dubbed 'content'. What pertains to identification, and

what to content, depends upon circumstances particular to each event. We must briefly consider each in turn.

(a) Guardian-spirit: The identity of the relationship was made manifest, after acquisition, at the next winter spirit dance, partly non-verbally, and specifically through song (a quotation from the vision experience). In native theory, there could be then no doubt on the score of IDENTITY. The validity of the CONTENT of the vision, the anticipated acts and experiences in later life, remained uncertain. Such content was entirely specific to the particular relationship and the one person's life, not within social control. Indeed, as with other American Indian groups such as the Crow, there probably was the possibility of being mistaken as to the import of a vision, or of some aspect of it. The moment of death, then, is the only moment when the validity of the content of the vision can be completely certain. (That verification seems always to have occurred is a matter, not so much for amusement, as for envy; the Wishram would seem to have provided a perfect pattern for an ultimate acceptance of identity which, according to Erik Erikson, is necessary for psychological maturity and health.) The content of the vision may also have been sometimes ambiguous, such that later events could have been held to clarify (rather than retroactively emend) it. In sum, disclosure of the content of the experience was proscribed outside the event constituted by the dying visionary's own first, and final, public disclosure.

(b) Personal names: The identification with the name was made manifest, after acquisition, in contexts of address (quotation from the ceremony). The ceremony of bestowal could itself be held to have settled the FACT of the relationship between the name and the new bearer, for both the individual and the group of kin in whom right to conferral of the name was vested. The validity of the CONTENT of the name, as a near-title, could not be known, so far as the future life of the new bearer was concerned. One could not be sure that the new bearer's life might not bring the name into disrespect, or even that the new bearer's line might not become extinct, and the name fall into disuse. At the time of the ceremony, one could be certain of these things only in retrospect. The prestige-content of the name was within social control only with respect to the life of the former bearer, and with respect to the ability of his descendants now to reconfer the name. Hence the emphasis of the ceremony upon the past (re the former bearer) and the present (re the reinvestiture). The former bearer's relationship to the name was both formally validated and formally terminated. The family's identity and status was formally validated, through restoration, or maintenance, of a valued property (recall that the name was not conferred until survival in a social identity was assured: to name was in part to assign to a type a new token). The new bearer's relationship to the name must wait for validation and termination until five years or more after death.

(c) Myths: The identity of the relationship between what was announced in myths and what was encountered in the present world was made manifest in quotation and allusion on various occasions throughout the year. Within the mythological period, the actions of the myths might be taken to have resolved uncertainty, as to the identity,

attributes, names, even existence, of plants, animals, persons, places, and things. Within the present period, to quote and allude to the relationship between acts in the mythological period and features of the present world ran no risk of disconfirmation. On this score, the myths and the world validated each other; and, over time, the fit was within social control.

Is there then no relationship between disclosure and certainty, between concealment and uncertainty? Part of the answer lies in the implications of the statement by French (1958: 260) that "myth materials are not intrinsically sacred or formally moral; they become so in context."

In short, one must distinguish between the myths, as MATERIALS, capable of quotation, and the myths as ENACTMENTS, quasi-ritual acts, disclosing not only details of the Wishram cultural world, but also the underlying values and beliefs of the Wishram as to their relationship to that world. It is the former aspect of myths that is independent of the tabu against the telling of the myths in summer; it is the latter aspect that is not.

The values in question can be characterized, as has been noted, as those of 'participant maintenance' (the Myth of Salmon is a salient example; cf. Jacobs 1955, Hymes 1958). The view of participant maintenance entails mutual respect and support between the human and the (super)natural worlds. Spier and Sapir catch its flavor in Wishram prayers:

Prayers were directed to the earth, the rivers, the clouds, to the whole category of natural phenomena. The impression derived is that the Wishram thought of themselves as among the earth's creatures, one class of things among the elements of the universe, and on a parity with them, no different from them. The prayers and declarations to the rivers, mountains, and whatever, seem to imply only that the individual wished to fix, to indicate his place among them. (236)

Success in life, for both individual and community, lay in maintaining the relationships proper to one's place. The natural powers were by nature beneficent, if men did their part in maintaining the natural order and did not violate its rules (did not, for example, kill more than was needed).

With these values in mind, notice that the period of concealment, of non-narration, of myths is summer, and that summer is the major period of subsistence activity, during which take place the gathering, fishing, hunting and trading on which survival through the winter depends. (We are here assigning the larger part of the year, including late spring and the fall, to 'summer'.) In one sense, as regards the details, the myths have laid down the character of the Wishram world for all time. In another sense, as regards the underlying relationships, what the myths have laid down is conditional, contractual. (Some myths make this explicit.) Disregard of the conditions could mean failure at hunting, failure at fishing, disastrous weather, even starvation, for an individual, or even for a whole community. (Some closing formulae for myths refer to good weather, and bad weather is a common punishment expected from disobeying the tabu against enacting myths in summer.) In summer, between

winters — sacred seasons — the relationship to the supernatural world was conditionally uncertain. During the winter, the fact of survival, and ceremonial activity, reaffirmed the relationship as having been successfully maintained during the preceding seasons of major subsistence activity.

Each of the three events can be seen as parallel in this last respect. In each the relationship in question (guardian spirit, name, mythical) may be implicitly manifest in activities during the period (a life in the first two cases, summer in the second) complementary to its formal disclosure.

(d) We have found the common principle to be quite simply this: disclosure depends upon certainty. The common pattern can be characterized as follows: Each event has two components. One can be quoted, after acquisition, outside the formal event. The other can be disclosed only within the formal event. What pertains to each, and the occurrence of each, depends upon what can be considered certain, and when, in terms of the institution of which the event is part. Thus, the period of uncertainty as to content for myth is a portion of an annual cycle; for naming and spirit experience, it is the better part of a life. For both the latter, concealment of content precedes, disclosure follows, a completed life. Analysis in terms of the underlying 'perfective' valuation shows all three events to form part of a common Wishram pattern.

To sum up: we have found three major Wishram speech events to share a pattern constituting them formal events; to share a pattern relating the formal event to the cycle of which it is part; and to share an ordering on several dimensions. The analysis has depended upon, and given evidence for, a Wishram valuation of use of language.

My hope is that the analysis can be taken as at least an illustration of the possibility of discovering structural relationships characterizing the use of language in a culture, and cultural valuations of language implicit in such use. The analysis is also an illustration of the possibility of using ethnographic data in essentially a philological and historical way for such a purpose.

The analysis can also be seen as an illustration of the use of a set of etic categories for the analysis of communicative events — categories for types of factors which should be considered in arriving at an emic (structural) analysis. I have elaborated such categories elsewhere, using the labels: communicative events, senders, receivers, channels, message-forms, codes, topics, settings. It can be seen that the preceding analysis deals with instances of all of these: the disclosure of the guardian-spirit experience, naming ceremony, myth narration (communicative events); the constitution of such events, involving a repeater (addressor), an original source (sender), and public (addressee); verbal disclosure of content, partly non-verbal disclosure of identity of guardian spirit (channel); conventionalized verbal makeup, and sequential ordering of the messages of disclosure (code); hierarchical relation of events as to scope of content (topic) and as to scheduling, frequency, and cycle of occurrence (setting). It can further be seen that the categories have not entered into the analysis in a mechanical way, as *a priori* boxes to be filled with lists. Rather, the categories have been used heuristically, and have remained implicit in terms proper to the data.



## CONCLUSION

I think it fair to say that differences have been sketched which need accounting for in terms of a theory of the use of language. Certainly some indication of differential role for language in several American Indian societies has been given; certainly the differences in role are pertinent to the sorts of difference associated with the first type of linguistic relativity. One suspects that the relation of language to world-view was different as between the Fulniô and the Guayqueries; we are assured it is different now between the Eastern Cherokee and the Hopi-Tewa. One suspects again a difference as between the Hidatsa and their close kin, the Crow. I have suggested even a parallel between the Wishram perfective future tense and the perfective valuation of the use of language.

Let me make clear the status of the last suggestion. I do find significant parallels between the linguistic habits and the other cultural habits of the Wishram. The perfective orientation is one; the usage as to change of names after death of a family member was cited earlier as fresh evidence of another parallel, concerning bipolar orientation, for which I have sketched evidence in a previous paper (involving recent distinctive changes in grammar and lexicon; Hymes 1961a). Both parallels lie at the first level considered in the initial discussion of the Whorfian type of linguistic relativity. They assert pattern, not causality. The patterns are correlations, indeed, of much the same sort as those summarized by Whorf at the conclusion of his 1941 article. The patterns have historical significance as products of the sustained common development of a language and the rest of a culture together; they indicate something of a fashion of speaking, and a cognitive style. How much they indicate of a cognitive world can no longer be determined.

These Wishram patterns, indeed, return us to the leading point of the paper. In terms of the second type of linguistic relativity, the perfective valuation is an encouragement; it is evidence of a serious and focal role for language, making credible arguments for the significance of the content of the language with regard to the contexts in which it is used. The status of the ethnographic and linguistic data, however, and beyond that, the known linguistic situation, preclude us from imputing to any present day speaker of Wishram any cognitive orientation or outlook derived from an analysis of his language. This is so, even though we might impute the orientation or outlook to users of the language in the recent past (mid-nineteenth century), when changes reflecting the outlook presumably were in process; and even though the data from which the inference would be made has not changed essentially between that period and now. For in role, Wishram is today the first language of none, the second or third language of few, and seldom used at all.

Here is the main point. Two sets of data from a language may both lead to an inference as to a distinctive cognitive style. Yet if the data is collected in abstraction from contexts of use, someone might infer a presumptive world view from both, only to find later that one set came from a speaker to whom the language was an argot,

learned late in life for commercial purposes only. No one would wittingly do that; the differential functional place of languages in multilingual situations is readily recognized. I have tried to indicate from ethnographic data that multilingual situations are but special cases of a general situation; that the patterns and specializations of the use of language in any society must be analyzed, as the indispensable basis for any other inference as to the place of language in personality, society, or culture.

One final observation: sociolinguistic systems disappear before their languages, perhaps several generations before. If salvage linguistics is urgent, salvage sociolinguistics is doubly urgent.

#### DISCUSSION

MATHIOT: I would like to make a comment not directly related to Dr. Hymes' differentiation of the two types of linguistic relativity, but concerning the need for differentiating between important factors. One such differentiation potentially relevant to Hymes' topic is that between linguistic categories and linguistic properties.

In my own work on the cognitive significance of language, I have observed that it is relatively less difficult to infer the cognitive content of linguistic categories than that of linguistic properties. I have also observed that the results one gets from the cognitive study of categories are different in nature from what one gets in studying properties. So from this personal experience — which is obviously limited — I am tempted to conclude that there might be an interesting difference between these two aspects of language, the categories and properties. This distinction may have some bearing on the study of linguistic relativity. By a linguistic property I mean a broad characteristic of the linguistic system as a whole. An instance of a linguistic property is fusion. There are different degrees of fusion in both the grammar and the lexicon. Examples of different degrees of grammatical fusion are single-base words as opposed to compounds.

HAUGEN: It seems to me we must clearly distinguish between the various parts of language in the way Fishman suggested. I found his article very informative, and it confirmed my own previous reactions to the Whorfian view, namely that if we say with Whorf that the linguistic structure influences the non-linguistic behavior of its speakers in some significant way, we may actually be saying several different things. On the one hand we all know that lexica are different, and that these in turn correlate closely with culture and therefore inevitably dominate the behavior of the speakers who use that language. But this seems to be rather a trivial point, since it is generally accepted and not particularly controversial. But an interesting and striking conclusion — and this is what I believe Whorf in some of his writings had in mind — would be to suggest that the structure itself, the grammatical and even phonological structure, had some kind of relation to non-linguistic behavior. Most such structures were established at a period of the language far anterior to the culture of the present-day speakers, as in the case of Indo-European, where many languages have a three-

gender system which obviously does not reflect anything in our present-day culture, yet we simply have to use it because it is part of the language. So, when you say here that the linguistic structure has not changed essentially from that period, it seems to me a refutation rather than a support of this more interesting aspect of the Whorfian hypothesis. With the same structure you can have two quite different world views, between a man who uses it as his first language and a man who uses it as his second language. We can speak French without becoming acculturated to French culture.

HYMES: This is a very important point, and my own view would be somewhat as follows: I would distinguish, both in Whorf's own work and in principle, between different kinds of inferences that one can make from linguistic structure to these other things. And it is extremely important in discussing Whorf to distinguish different statements he made in different contexts. My own feeling is that in some of his writings, which were intended perhaps to excite a more general public interest in these things, he simplified and wrote more strikingly and dramatically on the subject than in others. In the article which I think is his most important on the subject — the one in the Sapir Memorial Volume (1941) — the conclusion is a very conservative conclusion. It does not argue for direct connection of any particular aspect of linguistic structure with world view or patterning in the rest of culture, but for such a connection regarding what Whorf calls "fashions of speaking". Now as I interpret fashions of speaking (something that can also be called cognitive orientations or cognitive styles as manifest in active use of a language), one cannot prescribe in advance what features of a language will be involved. To study fashions of speaking involves the analyst in detecting consistency of patterning among different sectors of linguistic structure — Whorf mentions patterns cross-cutting lexicon and grammar in particular — which would seem to be active or productive in the particular language. Now, if this were the case, then one would avoid the problem of anachronism, inferring part of a particular world view from, say, gender in a language in which gender is a fossil. This is certainly always the position I have taken. In the Wishram case where I have made a similar kind of inference, the evidence which I stress has to do with linguistic formations which are new, in response to acculturation situations, or which are contrastive as between this dialect and others, so that we are sure that they have developed in the most recent period of this dialect, and presumably therefore reflect active outlooks on the part of the speakers rather than antiquated ones.

With regard to relation with culture, Whorf at the end of his essay (1941) is simply saying there are connections as historical products between Hopi language and culture in the respects which he has tried to point out, and that one wouldn't expect to find such connections unless the language and the culture had been together for a long period of time. Whorf seems to me to be saying at that point something wholly unexceptionable, namely that one can infer patterns connecting different sectors of the cultural habits of a people as historical products. When he is saying that, he is simply saying that as a result of past historical forces shaping these habits, there's a certain consistency among them. Anthropologists would just have to go out of

business if they couldn't admit that kind of inference. Now in principle, from my point of view, this need say nothing about present-day speakers of Hopi or present-day members of Hopi society. It is a fairly super-organic conclusion, so to speak. It is a conclusion about the content of the culture and the content of the language as they have developed together historically; it is not a conclusion about the influence of these on present-day members of the society in which they are manifested. If you want to know what effects these things may have upon a Hopi child growing up today, you've got to go out to the field and devise some experimental way or inductive way of determining this. So I distinguish very sharply between an inference as to connections of pattern between language and culture as historical products, on the one hand, and inferences from the existence of such patterns to the behavior of particular people who may speak the language now. And this is why I see the conclusion of my paper as only a partial refutation of some implications that have been drawn from Whorf's writings. That is, if anyone thinks that the simple presence of a pattern in a language and culture permits us automatically to make a certain inference as to the behavior of any speaker of that language, this I deny.

HAUGEN: Well then, that to me is simply a refutation of the more striking aspects of the Whorfian hypothesis. What I was thinking of was such exaggerated statements as that relativity could have been better expressed in Hopi than in Indo-European, which to me is the purest nonsense.

HOENIGSWALD: I would like to ask the speaker's opinion on an impression which I have often had. It seems to me that some Whorfians speak of world-view in two ways which it might be preferable to keep separate. They sometimes refer to attempts of the speakers of the language or members of the culture to engage in theory, however incipient and rudimentary, that is, to make things explicit, and to reason. And on other occasions they speak of features which are present in the culture and which need the anthropologist's analysis to be brought out into the open. Now it seems to me that in the first case the concept of dependence of those attempts at reasoning on language structure is a very real and demonstrable proposition; and it may actually turn out that it is generally true that attempts at philosophizing, that is to say again however simply and crudely, are in effect nothing else, in the reductionist sense, but language structure masquerading as something else. Therefore, dependence has here a very good and testable meaning. When it comes to the other thing, I don't know what the evidence is, I don't know what the meaning of the term 'dependence' is. I could only attach meaning to the term 'inter-dependence' as Hymes uses it in the beginning of his paper.

As far as gender in Indo-European is concerned, I am not sure that I understand either the speaker or Dr. Haugen when they wish to attach importance to the establishment of the feature rather than the existence of the feature. It seems to me that there may be a linguistic difference between the function of gender in the early Indo-European and a modern Indo-European language. In other words, it may be that gender in early Indo-European was a different thing from what it is now, and THAT

should be the phenomenon with which to correlate matters of world-view and behavior. But newness in itself seems to me to be not a relevant property.

HYMES: Well, a major piece of the Wishram evidence is of that sort, a shift in the tense-aspect system, which is now a different thing than it was. But if, for example, one has new words coined in a language which reflect a certain selective cognitive orientation, this is important evidence. If two American Indian languages, for example, in coining new names for the same objects, reflect distinctive patterns of perception of them in the names, I think this is relevant.

HOENIGSWALD: But then the newness is a descriptive property of the language. In other words, if you had only a synchronic picture of the language, newness would be a property which is detectable from the synchronic picture. New formation is perhaps a productive formation, or one which is not semantically isolated. Newness is a property of certain elements in the language, which stands out in the description. You need no accidental chronological knowledge to speak of.

HYMES: This I don't know, because it is usually approached in these cases from the other end. Namely, we know that the meanings must be new in the cultures in which these languages are spoken. So we then look to see how they are expressed. One wouldn't be able to pinpoint some of the forms as very recent without knowing that the meanings are very recent.

GUMPERZ: I have two comments. First, in connection with your remarks on Sapir's view of language function as a universal, I would like to point to his article on communication (1931). When talking about communication, Sapir follows Malinowsky and others in drawing a clear distinction between interaction in small primitive groups and in literate societies. This suggests that he does make a distinction between language and communication. Secondly, I am somewhat confused at the various uses of the word 'function' in the course of this conference. We have used the word to apply to domains of usage, i.e. home, public life, etc. Elsewhere, 'function' seems to refer to ultimate goals or ends of a message. I believe it is important to make a distinction between the two usages.

HYMES: What I remember from the article on communication is Sapir's saying that in a small group the common background would be great and therefore the explicitness of messages could be much less; the actual text of the message could be much less explicit and convey the same amount of information. In more diverse groups, for the same amount of information to be conveyed, the text would have to be much more explicit. This I think is very fundamental and important, but it is a point of a different order, perhaps, than the generalizations about the general role of language he makes in the essay on language. When Sapir talks about language as universally being felt as a perfect symbolism of experience, as a means of referential and expressive use, as a force for the development of individuality, etc., in the essay on "Language" (1933) — none of these statements seem to me to imply any necessary conflict with statements about functions of language in different groups. It's an empirical question as to what aspects of language function are universal and which particular to which types of group.

As for the word 'function,' it is a much abused, ambivalent, ambiguous word, and some sociologists have decided to do without it altogether because of this. I use it here simply because it is traditional in talking of this aspect of language, and I don't know of any other convenient single term.

MATHIOT: I want to try and answer Dr. Haugen's question (or challenge), and I'm going to defend Whorf. After conducting a very lengthy study, the aim of which was to study the world view of a people through language, I have come to the conclusion that most studies of the Whorf hypothesis have led to inconclusive results because people are trying to prove or disprove the hypothesis by going directly from language to behavior. And I think that the situation is much more complex than that. I think that one should not only consider the world view as inferred from the language, but also the world view that is reflected in nonverbal behavior. These are the variables that one needs when testing the possible influence of language — or a given aspect of language — on behavior. When Casagrade and Carroll tried to test the influence of the Navaho category of shape on the behavior of Navaho children, their work showed that the behavior of these children was not totally different from that of a group of Bostonian children who had been used to dealing with objects since infancy. They concluded that they could not really prove or disprove the Whorf hypothesis. It seems to me that what they have shown is the importance of the effect of cultural habits on behavior. The situation is much more complex than most people seem to think when they study the Whorf hypothesis.

FRIEDRICH: I agree with Madeleine Mathiot, and I'd go even further. I'm not sure that one has to prove or disprove the Whorf hypothesis in the sense of an experimental science. All of us are related to that abstract painting behind Dr. Bright right now, and I think one could develop very convincing discussions of that relationship, but I don't know that we want to prove anything, in the sense of a chemical experiment.

The main thing that I wanted to say, though, was to follow up on Dr. Hoenigswald's comment, by making it a bit more general and saying that in dealing with the Whorfian hypothesis we must pay special attention to the difference between the explicit culture and the implicit culture. What the linguist does a lot of the time is to infer implicit categories, especially implicit grammatical categories; and the perilous step, and often a total non sequitur, is to take the inferred implicit categories, assign them semantic meaning, and assign that to the cultural system. You can distill all sorts of metaphysical systems from many different kinds of grammatical statements. In fact, the implicit or inferred cultural systems differ from those which are explicit to the people themselves, the folk philosophies and the folk metaphysics, etc.

On the other hand, I think that Dr. Hoenigswald was right in saying that a great deal of philosophy, especially in its incipient stages, is a case of people asking what the words mean and especially what grammatical categories, endings, and connecting particles mean, and there are good descriptions and analyses already of this. The problem is that while we have rather elegant treatments of the obligatory grammatical

categories in American Indian languages, I don't know of any systematic analysis of the ontology and metaphysics of an American Indian system, and until we get some of those I don't see how we can do much correlating and relationship. It would be of course ideal to talk to a Navajo metaphysician, if there is such an animal, and in the process of reasoning with him see how he bases his arguments and to what extent he actually appeals to categories in the language, because I think he would appeal to them very often. But I would like to ask you how many good American Indian metaphysics do we have?

HYMES: Well, perhaps a few. John Ladd (1957) wrote on the structure of the Navajo moral code and Richard Brandt (1954) on Hopi Ethics, both stimulated in part by this problem; and both pay a great deal of attention to language. With regard to the Navajo in particular, Kluckhohn, of course, was very interested in Hoijer's work; he related it to his own interpretation of the values and world-view of the Navajo, and felt that the two fitted together somewhat from an independent basis. There has not been as much attention to this aspect of primitive Amerindian societies or non-literate societies in general as there should be. One thinks of Radin's work on *Primitive Man as a Philosopher* (1927), of course, but there is relatively little.

PAPER: I welcome Dr. Hoenigswald's very acute observation of the distinction of the two areas of meaning of 'world-view' here. But I would like to ask him or anyone else here if they can come up with some really good examples of this. Some of the more extreme Whorfians have spoken of language as DETERMINING the world-view, and there has been a kind of reductionism on the part of these people who suggest that Greek philosophy is there because it was the Greek language that people spoke, etc. And of course the most telling counter-argument is that it is precisely the speakers of Standard Average European who have developed these categories of physics and philosophy, which, as Whorf points out, are the ones that are the most difficult to talk about in Standard Average European. In the words of a now deceased linguist, "It isn't that the Hopi have a different physics from the standard average European speakers; it's that the Hopi have no physics at all." And without waiting for studies of Amerindian metaphysics, we have well-studied metaphysics in languages for which we have a longer documented history, and surely someone ought to be able to come up with some good examples that would nail this thing down.

HYMES: Well, of course the Hopi do have some physics in a sense, the ethnophysics or folk-physics, and Whorf's paper on the expression of vibratory phenomenon in Hopi, for example, is a very interesting case in point. But I agree with you, of course, on the obvious fact that, as I mentioned earlier myself, it was our society which developed that theoretical physics and not the Hopi, which is evidence enough that more is involved. I think this gets us back in a way to the point of my paper, which is that, underlying these questions in relation to the structure of the language and anything else, is the question of the role the language plays, and this may be different from one case to the next. Now if one were, for example, interested in what effect linguistic structure had on outlooks, in the sense of the makeup of the world, one would pres-

unably try to compare folk views of our own society with those of the Hopi, which would be the proper comparison, and not the outlooks of people who had been precisely trained to use the language in a particular way, for a theoretical investigation. Even there, according to some very respectable theories of science, one should not be surprised to find underlying a particular scientific position, and one valid for its time, a certain metaphor or implicit perspective on the world which turns out to be relative to the culture and not universally valid. This continually happens. As concerns the influence on philosophy, I am by no means expert in this field, but there exist among philosophers people who maintain this very proposition, who derived it independently of Whorf, and who have talked about the influence of linguistic structure on Aristotle's categories of logic, for example, with some apparent evidence for this (this would be in line with Prof. Hoenigswald's point) — and who have seen the wrestling with the linguistic means of expression as very important in the development of philosophy.

FISCHER: I just had a comment to make on Dr. Hoenigswald's point about new features being not necessarily of special relevance. I would agree with this, to the extent of saying that old features could also sometimes have continued relevance, although I also agree with Hymes that this is one way to look for features which might be some sort of special functional significance, special relatedness to current world view. But certainly you could also, by comparing related languages, find in some one language perhaps some very ancient features which have been retained only in that language, and these might also be of special interest.

HAUGEN: The Whorfian hypothesis of course wasn't Whorfian — there are articles by linguists going far back into linguistics which have maintained some such point. The Swedish linguist Esaias Tegnér wrote a whole book on the power of language over thinking. The Norwegian August Western wrote a reply called *The Power of Thinking over Language*. Since the two are obviously interdependent, it seems to me that a statement such as Dr. Friedrich made, that you cannot prove or disprove the Whorfian hypothesis, is the most damaging thing that has been said about it here today. This means that by definition it is not a scientific statement. If this is true, then we are dealing simply with an opinion, perhaps with an almost quasi-religious statement. The notion that language determines philosophy, which I have met again and again, e.g. that philosophy had to arise among the Greeks because of the structure of the Greek language (and there are very distinguished linguists who will maintain this) seems to me to be skating on thin ice. In fact, philosophy did arise there, but whether or not it might have arisen elsewhere, we'll never know. As for relativity being statable in one language rather than the other, my impression is that relativity is not statable in any natural language whatsoever. It is statable only in mathematics. No language can state a concept as complicated as this, and there is no reason to expect it to. Only by the development of mathematics were we able to get away from the weaknesses of our natural languages and the limitations which they impose on our world view, which we are all willing to grant.



To me the Whorfian hypothesis is questionable and a bit repugnant when stated in its extreme form, simply because it denies by implication its own opposite, namely that all languages are translatable into one another. And this I believe to be also a valid statement. All languages are translatable one into the other. If this were not true, the activities of the Summer Institute of Linguistics, for instance, would be not only difficult but impossible. On the one hand translatability is highly limited; on the other hand the independence of individual languages is limited. Neither is perfect, and we struggle with both. Any language that cannot express a given philosophical term can either borrow it or make up its own equivalent.

HYMES: I think you are expressing Whorf's position! I've argued this in a book called *Studying Personality Cross-Culturally* (1961b). People often forget, in criticising Whorf, that he talks about language in relation to HABITUAL thought and behavior. There's a very important difference between that and what people can potentially do, given their capacities as human beings undressed by language and culture so to speak. The fact that they are naming and using language as they go through every-day life forces them to rely upon the habitual categories of the language in any particular circumstances.

HAUGEN: Right now I'm talking about philosophy — not habitual daily usage.

HYMES: Well, philosophy is ultimately the daily usage of philosophers. When philosophers work in the analysis of philosophical problems, they are using a language to a particular purpose and in a particular context. I think the English philosopher Collingwood's autobiography (1939) has a very illuminating chapter on this particular point, on the problem of question and answer as a logic in philosophy. But that aside, I think the point is that Whorf was not maintaining that people could not transcend the patterns of a particular language; in fact, I think he wrote his articles hoping that people would. If one reads all of Whorf's articles, and particularly the last one in his selected papers, one sees that he in fact believes in universals of thought, however he expresses them. If one reads his particular statements, saying that the people in communication will have problems unless they can calibrate linguistic backgrounds, I think this is the underlying point. Whorf is trying to call attention, is in fact arguing that the role of linguistics is to call attention to these differences in structure, so that people will be able to transcend limitations that these particular patterns may place upon their behavior in ordinary daily life.

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# SYNTAX AND SOCIAL STRUCTURE: TRUK AND PONAPE

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## 1. INTRODUCTION

This paper discusses some syntactic differences between two Micronesian languages, Trukese and Ponapean, and proposes social and cultural factors which may have been responsible for the divergent syntactic developments which are noted. The languages and cultures of Truk and Ponape are genetically connected but show a number of differences which have presumably developed under geographical isolation for a period of two or more millennia. (The two languages share about 40 % of the 100-word Swadesh list; applying the previously determined retention rate of .86 per millennium for this list would yield a separation date of about 1,100 B.C.)

Data on the languages and cultures are derived from fieldwork during the period 1949-1953 when I was employed by the U.S. Trust Territory government in the Eastern Carolines. This fieldwork was supplemented by the published and unpublished studies of other investigators which I have been able to consult. Of course, it should not be assumed that these other investigators would necessarily approve of the phrasing of statements in this paper; in some cases I have reason to believe they would not. For the languages, Elbert's Trukese dictionary (1947) and the unpublished grammatical sketches of Trukese by Dyen (1948) and of Ponapean by Garvin (1949) deserve special mention. For social structure and related topics, the monographs of Gladwin (1953) and Goodenough (1951) on Truk and the unpublished reports of Bascom (1946) and Riesenberg (1949) on Ponape were particularly helpful. I acquired a speaking knowledge of both languages through transcribing numerous texts and using both languages daily for three to four years each.

The term 'sociolinguistics' is most often applied to studies of linguistic variation within a single society and culture in response to differences in situation, speaker-listener relations, social class, etc. Investigations of differences between two languages are usually regarded as studies of 'language-and-culture'. However, the present paper might fall under either category — sociolinguistics or language-and-culture — since it involves the assumption that differences which have arisen in the two related languages are produced by certain long-standing differences in general features of the social structure of the two speech communities.

Incidentally, these differences in social structure may be further related to geo-

graphical differences' in the 'arenas' of the two societies. The possible influence of geography will not be discussed here except to mention that Ponape is a single unified land mass, larger in total area than Truk, and more remote from other inhabited islands; while Truk is a complex atoll (a number of small high islands inside a large lagoon) which is within easy sailing distance of two chains of populous low islands. With a neolithic technology of roughly equal effectiveness in both places, it would be easier on geographical grounds for a larger society to form on Ponape than on Truk, and there is evidence that this is what actually happened.

The societies, languages, and cultures of Truk and Ponape have many common features, which are too numerous to describe here. This paper will instead concentrate on certain differences between the two cultures, primarily with respect to syntax and secondarily with respect to social structure, which is regarded here as having influenced the direction of syntactic divergence. Nevertheless, it should be kept in mind that the similarities in both language and social structure are pervasive and basic.

Since language appears to be an especially conservative aspect of culture, with roots going far back into the past, studies of language and social structure must face the question of cultural lag. If we study the relation of linguistic form to society or culture, considering a single non-literate culture at a time, we run into considerable difficulties in trying to match existing linguistic forms of varying age with the various earlier stages of society and culture in which they arose, unless we make the comforting but probably naive assumption that the adjustment of linguistic structure to changes in society and culture is practically instantaneous. Comparisons of two unrelated cultures and languages, such as our own as against some exotic culture, shed but little more light than does a one-culture study, for even if the two societies can be shown to have recently converged in culture, one would still expect the languages to be different as a result of having started at different places, so to speak. One methodological solution is to compare two related languages which are descended from a common ancestor. The only differences which will then be found will be those which have developed since the two speech communities become separated. If influences of social structure on language do exist, they should be involved in these rather limited linguistic differences. The problem thereby becomes vastly simplified and, in theory at least, manageable.

It is easy to see how areas of abundance and sparseness in the lexicon can be related to cultural elaboration and also to internal social structure, insofar as there are special segments of the community which possess special vocabularies. However, this relationship between relative differentiation of lexical areas and sociocultural emphasis on areas of activity is somewhat lacking in challenge just because it is so obvious, although much further work could still be done usefully along this line with high-speed computers.

Studies of the relationship of syntax to social structure and culture have been less frequent, and less satisfactory when they have been made. Some of Whorf's studies (Carroll 1956) were not limited to vocabulary differences alone, but even so Whorf

emphasized in his writings the significance of grammatically required categories signaled by particular morphemes, whether affixes or semi-independent particles. The seeming arbitrariness of syntax suggests at least that the influence of social structure on syntax, if it exists, is not all-powerful and may be rather slow-acting. At the same time, if we are to develop a theory of change in syntax, it will probably be very helpful to begin by examining pairs of related languages which show relatively small and manageable differences and trying to devise explanations for differences of this magnitude.

The following sections contain a description of some differences in syntax between the two languages — principally differences in the construction of noun phrases. Then comes a description of some differences in general social structure which are considered to be of possible relevance to the syntactic differences. Finally a possible explanation of the linguistic differences is offered, in terms of differences in cultural psychology as influenced by social structure.

## 2. SYNTACTIC DIFFERENCES: NOUN PHRASES

Grammatical differences between the two languages are not numerous. There are some differences in morphology, of which the most striking is that Ponapean has more complex verb suffixation. In syntax the most striking differences are connected with the formation of noun phrases and substantives, and it is these which will be reviewed here.

In both languages noun phrases may be formed by the combination of a noun with another noun or with members of a number of other distinguishable form classes which modify the noun. These other form classes include (in both Trukese and Ponapean) adjectives, demonstratives, ordinary quantifiers, interrogative quantifiers, numerical classifiers, and possessive classifiers.

The noun in a noun phrase may occur in either of two forms, suffixed or unsuffixed. There are two types of noun suffixes: the possessive suffixes (corresponding to the personal pronouns) and the construct suffix, *-n* or *-Vn* in both languages, generally translatable in English without change of word order as "of" (e.g., Trukese *fönü* "land"; *fönü-en* ... "land of ..."). Possessive suffixes may be attached only to certain nouns, most notably those signifying kinsmen, parts of the body, and intimate personal possessions. Possession with other nouns is indicated by a separate word. The construct suffix may be attached to any noun, and the resultant combination is always followed by a word or phrase governed by the suffix. In the table below, noun phrases in which the head is a noun with the construct suffix are not listed separately since they can be treated identically with unsuffixed nouns, although nouns with a possessive suffix must be treated separately at one point.

The position of modifiers of nouns in a noun phrase is somewhat variable in both languages. There are differences in position within each language between some of the form classes of noun modifiers, as well as differences between the two languages for

certain form classes. However, the most common position for modifiers of nouns in both languages is following the noun. This is the invariable position for words or phrases governed by the construct suffix, for adjectives, and for relative clauses. Since the following position is always used for modifying phrases of any length, we may consider it to be in some sense the 'normal' position in both languages for modifiers of the head noun in a complex noun phrase. Nevertheless, for the other types of one-word noun modifiers mentioned above there is the possibility or necessity in at least one of the languages for the modifier to precede the noun in certain circumstances. These possibilities may be summarized in Table I.

TABLE I

*Differentiation of One-Word Noun Modifiers by Position*

<i>Type of word</i>	<i>Position</i>				
	1	2	3	4	5
Demonstrative with suffixed noun	—	—	—	—	TP
Demonstrative with unsuffixed noun	T	—	—	—	P
Interrogative quantifier	—	—	T	—	P
Regular quantifier	—	—	T	P	—
Numerical classifier	—	T	—	P	—
Possessive classifier	TP	—	—	—	—

*Note:* T — Truk; P — Ponape. Position 1 — modifier always precedes; 2 — usually precedes; 3 — may either precede or follow; 4 — usually follows; 5 — always follows.

It will be noted that positions 2, 3, and 4 in the table involve the possibility of variation within the language. These variations are a matter of emphasis and stylistic preference of the speaker in a particular situation. Where there is a choice, the preceding position is the more emphatic. It will also be noted in the table that in the four cases where the two languages differ on positions allowed or preferred, it is Trukese which gives greater preference to the preceding position.

In both languages there are independent substantival forms which correspond to all the types of noun modifiers listed in the table and which can be used in place of nouns if the context makes their reference clear. Some of these substantival forms are distinguished from the noun-modifying, or adjectival, forms by their greater length. In these cases the adjectival forms may be conveniently regarded as abbreviations or contractions of the substantival forms. Thus, for example, the Ponapean quantifier which may be glossed in English as "some" has the form *ekei* as an independent substantive, but the form *kei* as a noun modifier in final position. In other words, in a case such as this, there is an overt phonemic differentiation of the adjectival and substantival use of the morpheme in addition to the differentiation provided by the structure of the sentence and context of the utterance. There are more such overt differences in Ponapean than in Trukese, as Table II shows:

TABLE II

*Phonemic Differentiation of Adjectival and Substantival Forms of  
One-Word Noun Modifiers*

<i>Type of word</i>	<i>Same forms</i>	<i>Separate forms</i>
Demonstrative with suffixed noun	—	Truk, Ponape
Demonstrative with unsuffixed noun	Truk	Ponape
Possessive classifier	Truk	Ponape (partly)
Numerical classifier	Truk	Ponape (partly)
Regular quantifier	Truk	Ponape (partly)
Interrogative quantifier	Truk, Ponape	—

In both languages a form class of adjectives may be distinguished. One way in which adjectives may be used as substantives is in combination with a preceding nominalizing particle (Trukese *mei*, Ponapean *me*). A noun modified by an adjective constitutes one type of noun phrase in both languages. There is a slight difference in the two languages, however, in that in Trukese a noun phrase containing an adjective modifier most commonly consists of the noun plus the nominalizing particle plus the adjective (e.g., Trukese *cuuk* “mountain”, *tekia* “high”, *cuuk mei tekia* “high mountains”), while in Ponapean the nominalizing particle normally does not intervene between noun and following adjective (e.g., Ponapean *nahna* “mountain”, *ileile* “high”, *nahna ileile* “high mountains”). However, the nominalising particle is inserted in Ponapean when the adjective itself is modified by an intensive (e.g., *inenin* “very”, *nahna me inenin ileile* “very high mountains”).

In both languages there are also noun phrases which consist of a noun modified by a following relative clause containing a verb. In Trukese the relative clause can always constitute a major sentence in itself if lifted out of context and adjusted in intonation. In Trukese, both relative clauses and major sentences containing verbs always contain a verbal subject pronoun. In Ponapean only, however, relative clauses always begin with the nominalising particle *me*, here serving as a relative pronoun which may represent any component of the clause: subject, object, etc. In Ponapean, when the subject of the relative clause is the nominalizing particle *me*, there is no subject pronoun or further substantive as subject to go with the verb. Because of this use of *me* as a relative pronoun, a Ponapean relative clause removed from context could not constitute a major sentence in itself, but would instead simply constitute a substantive phrase which could be used as a substitute for a noun.

We may summarize the differences between Trukese and Ponapean noun phrases by saying that the Ponapean noun phrases are constructed more tightly than the Trukese, where there are differences. In Trukese there is often some ambiguity as to whether the modifier segment in the noun phrase is really attached as an attribute to the noun head rather than being simply another substantive in apposition. In the case of nouns modified by relative clauses, it is true that the Ponapean relative clause is



capable of interpretation as a substantive phrase in apposition, but the Trukese relative clause is even more separable and self-contained, since it is subject to interpretation as an independent major sentence.

In context these ambiguities are usually resolved, but I believe they still mean that there is a greater chance in Trukese than in Ponapean for the listener to decide that the speaker has come to a point at which he may be interrupted, when actually the speaker intends to say more. To give a simple example, if a Trukese speaker intended to utter the phrase *ewe iimw* "that house" and happened to hesitate after *ewe* "that" — and such a hesitation is not infrequent — the listener could assume with some reason that the speaker had really finished his intended utterance if the listener already understood the reference of *ewe* to *iimw* "house" from the context. The listener might then interrupt and take over the conversation. In the corresponding Ponapean phrase, *ihmw o*, the word *o* "that" follows the noun, is normally joined to it closely in intonation, and would only rarely subject to separation by hesitation in speech. Moreover, even if such a rare pause should occur, the listener would be normally led by context to anticipate some following demonstrative as a necessity. He would, therefore, be 'compelled', so to speak, to wait for the following modifier.

### 3. SOCIAL STRUCTURE

In a number of ways Ponapean social structure is more differentiated than Trukese social structure; that is, there is a greater variety of significantly different social roles on Ponape. Perhaps because of acculturation, this is not so obvious with respect to technological roles, but it is obvious with kinship roles and even more so with political roles. The Ponapean political roles are not only numerous and distinct, but are subject to important change due to individual initiative, so the Ponapean social structure is also more flexible.

One way in which the kinship roles are more finely differentiated in Ponape is with respect to genealogical distance within the matrilineal clan. Both societies have named matrilineal clans which are subdivided in practice into components of varying levels or degrees of inclusiveness. For analytical purposes Goodenough has distinguished five levels of matrilineal groups on Truk: descent line, lineage, ramage, sub-sib, and sib or clan (Goodenough 1951: 65-91). However, the Trukese themselves distinguish only two levels in their terminology: a lower level, variously known as *cóó*, *eterenges*, or (now more commonly) *faameni*, from English "family", and a higher level known as *einang*. The first three terms are not used contrastively for different levels of organization. All of these terms may apply to groups on several of the levels distinguished by Goodenough; but when they are used together, *einang* always refers to the more inclusive grouping and the other terms to the less inclusive groupings. Only the clans (sibs) have proper names on Truk; component groups do not.

The Ponapeans distinguish one more level of matrilineal organization in their

terminology than do the Trukese. The Ponapean terms are *peneinei* for the lower level groups, *kaimw* (literally "corner") for subclan, and *sou* for clan. Both clans and subclans bear proper names on Ponape. It is thus easier for Ponapeans to make fine distinctions between matrilineal relatives according to genealogical closeness. That is, the Ponapeans can more easily distinguish an intermediate degree of matrilineal relationship, the subclan, which contrasts terminologically with both the larger clan and the smaller lineage.

Both societies have ranking of family members in terms of seniority. However, in Truk the members of a functioning lineage are ranked simply by chronological age, so that the older person is invariably regarded as more senior. The principle of chronological age is also of some relevance in Ponape; but it is balanced by a principle of seniority of birth order in the line of matrilineal descent, regardless of chronological age. Thus the son of a woman who was the firstborn child in her group of siblings would have higher rank for purposes of inheriting family titles and property than his mother's younger brother, although the latter would nevertheless usually be chronologically older. The application of simple chronological seniority in Truk, as long as a matrilineal group remains united, results in the equation of all matrilineal relatives who are about the same age. The application of birth order seniority in Ponape results in differentiation of rank even among age-mates, and in a unique hereditary ranking for every member of the family.

Ponapean parent-child relationships add a further complexity to the differentiation of siblings which is not as marked on Truk. Specifically, the Ponapeans consider that parents are likely to develop preferences for particular children regardless of rank. These children will then be treated as *seri soaisoai* "favorite children", to the envy of their siblings. I believe that these favorite children are often children who, the parents hope, have special talents for advancement in the political title system, discussed below. Trukese may also have a special term for "favorite child", but if so I failed to hear it in speaking the language for four years.

The aboriginal political system on Truk was rather simple. There were local chiefs, whose position was hereditary in the matrilineal line. Assisting the chiefs were one or more officials known as *itang*, who had undergone a special training and served as orators and consultants to the chiefs on matters of mythology, war, and external relations. The *itang* were supposed to be either members of the chief's clan, or (according to some informants) his sons. In some parts of Truk Lagoon, at least, there were higher level chiefs who held a sort of authority over several nearby local chiefs.

The aboriginal political system on Ponape was in theory at least extremely complex. There was a fairly large number of "lines" or "paths" (Ponapean *ahl*) of ranked political titles, each title being unique and each line containing several to a dozen titles. The lines themselves were ranked and interlarded so that a grand over-all series for all the titles of a petty state could in theory be produced by someone who knew enough about the title system. Titles belonged to two levels of political entity. The chief of the local community or 'section' was empowered to grant traditional local

titles as he saw fit, whereas the two top chiefs of a petty state consisting of a number of local communities could bestow traditional state titles as they saw fit, using their own discretion in the application of certain accepted principles for the award of titles. The main principles for award of titles are as follows: 1) Those who are close matrilineal relatives of the highest chief are the most appropriate candidates for titles in the highest line; they should receive titles at an early age and advance rapidly in the system. 2) Those who serve the community and the high chiefs well are entitled to special consideration. 3) Those whose matrilineal predecessors have attained to a certain title are especially appropriate for receiving the same title. 4) When a higher title in a line is vacated, those below should all move up by one rank. 5) Finally, the better titles should be distributed to some extent among different factions and clans in the community.

These are probably not the only principles invoked, but they include the most frequent. As can be imagined, in any particular decision on a title there is usually some conflict of principles; there is no way of resolving this except through the more or less arbitrary decision of the highest chief. The system not only results in a unique named political status for each adult man at any particular time, but it produces a society of competitive status-seekers, each trying to outdo his neighbor and even his brother and to get in favor with the higher authorities so as to get a quicker and greater promotion.

The actual specific functions assigned to the holders of most of these titles are slight or non-existent at present. Probably in the past some of the titles represented actual functionaries at the chiefs' courts, while others may have represented ritual functionaries, such as the Master of the Sea, the Master of the Banana Plantations, etc. But perhaps many of the titles have always been more or less 'empty'.

Nevertheless, the titles do have significance even today, in that they govern the distribution of food at feasts and govern the public deference which an individual receives from others. Ponape traditionally had a large number of regular and special feasts, some of which are still observed. At a feast, food is presented to the chief by most of those present. This food is then redistributed among the participants, but in proportion mainly to rank, not to original contributions. After the highest chief present takes what he wants for himself, he tells an experienced subordinate to divide the bulk of the food among the participants. The higher title-holders present are then called in order, and the food is distributed to them in order of their rank, considering both the line of titles to which they belong and their position in the line. In general, those who rank higher get larger portions of food.

A participant who contributes much to a feast does not necessarily get his reward at that feast, but his contribution will be noted and he will receive more credit toward promotion than someone who contributes little. As the system operates, then, it works out to be something like a system of old age insurance, since an old man who has worked hard in his youth eventually receives a high title and can then sit back and enjoy life.

The total effect of the political and kinship systems of the two cultures is to produce one society (Ponape) where individual mobility, the uniqueness of the status of each individual, and his relative ranking compared to others are recognized explicitly and emphasized, as against another society (Truk) where change of status is little affected by personal effort, where unique individual status is of little concern, and where relative rank among individuals of the same age grade is minimized.

#### 4. LANGUAGE, PSYCHOLOGY, AND SOCIAL STRUCTURE

A bridge between language and social structure may be found in psychology. On the one hand, a particular social structure requires or facilitates certain general habits of thought. On the other hand, these prevalent habits of thought come to be reflected in the language, first in the lexicon but ultimately to some degree in morphology and syntax as well, since the language gradually becomes adapted to expressing in the most efficient manner what the majority of its speakers most need or wish to express.

The differences in habitual thought patterns between Truk and Ponape may be expressed in terms of several related polar contrasts, respectively concrete versus abstract, focal versus lineal, and stimulus-determined versus goal-directed or purposive. Dorothy Lee's contrast between Trobriand culture and behavior as 'non-lineal' and Euro-American culture as 'lineal' is, I believe, related to these, with the Trukese being more like the Trobrianders and the Ponapeans more like Americans (Lee 1959: 105-120).

Of course it should be understood that these are polar contrasts of ideal types. There are no cultures in which the participants engage only in abstract thought or only in concrete thought, for instance; this is always a matter of degree. Moreover, in any culture individuals shift back and forth from relatively abstract to relatively concrete thought depending on the situation. But it is at least a meaningful proposition that the average level of abstraction of thought in some cultures is lower than in others, as it is for some individuals within a single society.

Again, all conscious behavior presumably involves a set of stimuli and one or more purposes or goals. But in some situations a given familiar external stimulus may immediately set off a prescribed train of action, with little prior reflection or doubt and with considerable certainty as to the outcome; if the expected outcome does not materialize, the actor may simply give up and turn his attention elsewhere. In contrast to this, in other situations a person may conceive of a goal which he wishes to achieve and may spend considerable time and effort in investigating possible courses of action, shifting freely from one course of action to another as he encounters obstacles, until he achieves his goal. It is the first kind of behavior which is more stimulus-determined, while the second is more goal-directed. Both kinds are always found to some extent in any individual, but presumably there can be more emphasis on one or the other in particular individuals and particular societies.

Concrete, non-linear or focal thought and stimulus-determined behavior are especially adaptive in relatively small, simple, homogenous societies with low social mobility and stable cultures. For one thing, small non-literate societies are limited in the amount of cultural information which they can store up in the minds of their members. It is impractical to develop a large number of specialties, not only because subsistence techniques will not provide enough surplus food, but also because such a small society cannot be divided up into specialist groups of large enough size to preserve and transmit really distinct major sub-cultural traditions. Every member of the society, therefore, must have the most useful and time-tested answers to all the most frequent problems of daily living in his own head, or in the head of someone in his household. From the point of view of mental economy he cannot afford to store up personally many alternative answers to all these problems, and he also lacks the alternative of running to assorted specialists or books — an alternative which is open to members of large, complex, literate societies. In this sense of not being presented with major alternatives, the behavior of the man from the small society is more stimulus-determined than that of a more civilized man, although it may still be true that the former still makes many small decisions which are considerably more subtle and more finely adjusted than those of his civilized counterpart, who is more frequently confronted with alternatives of major consequence such as what town to live in, what profession to choose, what organization to work for, etc.

It will be recalled that of the two cultures under primary consideration in this paper, it is Truk which more closely approaches the model of a typical small society. Purposive, goal-directed behavior is more emphasized among Ponapeans, especially since their political system offers considerable scope for choosing alternate major goals and then choosing between a variety of often devious courses to achieve these goals. These choices are important to the individual Ponapean, since they can have great effect on his career, prosperity, and security. Moreover, though this is hard to establish conclusively by current ethnographic observation, I believe that before western contact there was more occupational differentiation in Ponapean society than in Truk, apart from the largely honorary political titles on Ponape. As already mentioned, the aboriginal political units in Ponape were considerably larger than those in Truk, making possible a greater variety of specialties with enough personnel to maintain a viable specialist tradition.

In the typical simple society, individual personality differences based on constitution and special life history remain, and may be even more sensitively appreciated than among ourselves, but they tend to be restricted to expressive behavior. In addition, differences in practical roles and practical cultural behavior are much less significant than among ourselves. In matters of everyday life, then, it becomes possible for a member of such a society to share much more of his knowledge with his fellows (making it practical also, incidentally, for the anthropologist to interview a limited number of informants). In such a situation abbreviated ways of speech are favored for communication about frequent shared events, and what are frequent events for

anyone in the society are in fact frequent for the others as well. Where several words might be required to express an idea to an outsider, only a single word might be required for a member of the in-group. The lexicon of everyday living becomes packed with meaning. Words and utterance become concrete in the sense that they express and communicate much detailed information with formal economy. Lee's discussion of Trobriand yam terminology is an example of this (1959: 90), although neither she nor I would restrict the effects to lexicon.

The assumption of shared knowledge in the small society also permits more 'pre-phrasal' use of words. There is a tendency to think that it is only pronouns which are used in place of other longer words and phrases, but in fact many words after the initial utterance in a continuous discourse take on added contextual meaning through reference to preceding words and phrases, in a fashion analogous to pronouns. It is this which I mean by the 'pre-phrasal' use of words: it is a way of TEMPORARILY giving words added meaning, of making the meaning more concrete, more detailed. Moreover, the more concrete is the general level of vocabulary in a discourse to begin with, and the more the speakers can assume that the listeners share much of the same detailed knowledge, the more words tend to grow even more concrete in realized (communicated) meaning through pre-phrasal use. These conditions, as suggested, apply with special force in simple, homogeneous societies.

As societies become more complex and social roles become more differentiated, the realized meaning of words in particular contexts becomes less important than the common or basic meaning. Speakers are forced to assume a greater cognitive gap between themselves and their listeners. At the same time, the basic meaning of the items of the lexicon tends to become more abstract and attenuated, since speakers have less need for words which can express much meaning in compact form to listeners who are conceived of as being much like the self; they have more need, instead, for words which can be used in many different contexts with many different listeners who are conceived of as being different from the self and from each other. Of course, it is still necessary to speak precisely about detailed matters much of the time, but this kind of concreteness can be achieved through the combination of several words which in themselves are relatively abstract. To use an example from Malinowski via Lee, where we might say "yam perfect in shape but small", the Trobriander would say simply *yogogu* (1959: 90). Certain common propositions in the complex society may now take longer to utter — their expression may be formally more complex — but at the same time there is a counterbalancing gain in flexibility and explicitness. The greater social differentiation in Ponape has to some extent produced a trend in this direction, noticeable when compared with Trukese, although compared with modern English Ponapean is still a relatively concrete language.

How, specifically, do the differences between Trukese and Ponapean reflect the polarities of concrete and abstract thought, stimulus-determined and goal-directed behavior? It has already been suggested that the construction of the Trukese noun phrase is looser, in the sense that noun modifiers are more subject to separation from

head nouns and to consequent misinterpretation as grammatically coördinate or independent. For one thing, this may be regarded as a sign of relative willingness on the part of the Trukese speaker to be interrupted. Rephrased, it is a sign of lack of concern with pushing on at all costs to the end of the sentence. Since the 'generalized other' on Truk is more likely to be conceived of as similar to the self, there is perhaps the feeling that the listener can probably guess the end of the sentence anyway, given a fair part of it. I would regard this attitude as relatively stimulus-directed, where speech is treated more as a response to an earlier utterance or to something in the non-linguistic situation than as a purposeful attempt to influence the listener in accord with a preconceived goal of the speaker.

The tighter Ponapean construction offers less encouragement to interruption. It suggests that the speaker has a definite idea in mind which must be communicated in full as a unit to the listener. It suggests further that the speaker assumes the listener to be perhaps quite different from himself and liable to misinterpret fragments of the full proposition. Speech of this sort I would regard as relatively goal-directed.

The concrete/abstract distinction also is reflected in rules of noun phrase construction. Much of the greater separability of noun modifiers from heads in Trukese is the result of the fact that a preceding noun modifier is subject to misinterpretation as a substantive. Thus, for instance, if the two words in the Trukese phrase *rueföc waa* "two canoes" are separated by an optional pause, the listener might be led to infer that the word *rueföc* "two long objects" was intended as a substantive, and to conclude that it referred in the particular context to "canoes". This would be a correct pre-phrasal interpretation of the word *rueföc*, and would result in assigning considerable additional meaning to this word, thus making its meaning more concrete in this context. The cognate Ponapean phrase *wahr riapwoat* is not liable to this misinterpretation to the same degree. For while the word *riapwoat* "two long objects" can be used independently as a substantive (in the same way as Trukese *rueföc*), in this phrase it can only be a modifier for *wahr* "canoe"; the listener thus has no reason to assign directly to it any more meaning than "two long objects". Possible misinterpretation is avoided even more clearly in Ponapean when the modifier and related substantival form are phonemically distinct, as is more often true in Ponapean than in Trukese. In brief, the argument is that, where cognate modifiers with similar meanings exist in the two languages (disregarding possible cultural differences in abstraction in the core meanings of nouns and their modifiers), Trukese tends to assign more concrete meaning to these modifiers in actual utterances than does Ponapean; and that the concrete, pre-phrasal meaning becomes assigned to these modifiers because of the more ambiguous or looser construction of Trukese noun phrases.

## 5. CONCLUSIONS

These differences in syntax between Trukese and Ponapean are not great, but I believe they are consistently and significantly in the same direction. They can be re-

garded, moreover, as consistent with other rather slight differences in syntax, morphology and phonology not presented in this paper. I do not see how these particular divergences in linguistic development can be explained in terms of psychomotor economy of speech alone, although I think that such an explanation would be appropriate if the entire social and cultural context were taken into account at the same time.

The explanation in this paper of syntactic differences between two related languages has been in terms of social differences which favor a different balance between two polar types of thought habits, variously labeled as 'concrete' versus 'abstract', 'stimulus-determined' versus 'goal-directed', 'lineal' versus 'non-lineal' or 'focal'. These pairs of distinctions are perhaps all closely related and may be simply names for different aspects of the same basic phenomenon. However, there may well be other variables of general thought patterns which are relevant to syntax and are functionally unrelated to the complex of variables discussed here. If so, clues to their nature may be found by careful consideration of small groups of related languages in relation to the social structure of the speech communities.

#### APPENDIX<sup>1</sup>

Hymes' discussion in his paper of the role of language in a society involves, at least in part, what I have referred to above as the expression in speech of prevalent habits of thought in a society. However, Hymes emphasizes a crucial link in the causal chain by dealing specifically with attitudes toward situations of speaking, rather than with all widely prevalent attitudes toward society and the world. In order to facilitate comparison between his paper — especially the section on Crow and Hidatsa — and mine, I give below a comparison of Truk and Ponape regarding typical attitudes toward speaking and in the functions of language.

1. *Traditional narratives.* In Ponape there is a greater development of sacred myths and songs which are highly valued; in Truk there is more emphasis on secular tales and songs which are considered to be trivial and to have only entertainment value.

2. *Ethnoscience.* Ponape has more supernatural sanctions for customs and more miraculous explanations. The Trukese take a more "common-sense" attitude toward their customs and toward natural phenomena, and are generally less concerned with justifications and explanations.

3. *Fluency vs. precision in speaking.* Ponapeans value concise, well-chosen speech. Trukese value loquacity and fluency, and are less concerned with careful choice of words. Stuttering appears to be more of a problem in Truk than in Ponape. In spite of a generally greater emphasis on personal achievement in Ponape, there is a greater emphasis on verbal productivity in Truk.

4. *Recital of magical formulae.* Exact repetition is emphasized in Ponape; while

<sup>1</sup> These comments were stimulated by Dell Hymes' paper, "Two types of linguistic relativity", and were submitted after the close of the conference.



repetition with great expressiveness of intonation, regardless of detailed accuracy, is emphasized at times by Trukese.

5. *Overt expression of emotion.* The control of emotion in public is greatly emphasized in Ponape, while relatively free expression of at least some kinds of emotion is allowed in Truk.

6. *Joking, lying.* Ponapean joking is milder and more controlled, while Trukese joking is more aggressive and freer. For instance, it is common to hear one Trukese tell another that he is lying, and this is usually a joke. Ponapeans make this charge much less frequently; when they do, it is more likely to be taken and intended as a serious insult.

7. *Etiquette.* Formal etiquette is highly developed on Ponape. This includes the use of two or more levels of respect vocabulary toward chiefs and other social superiors (cf. Garvin and Riesenbergs 1952). In Truk, respect vocabulary is weakly developed. Such respect behavior as exists is about as strong toward matrilineal relatives of the opposite sex as toward the highest chiefs, perhaps stronger; in other words, even the existing respect behavior on Truk is not deeply involved in social stratification in the usual sense of the term.

8. *Mode of addressing adult males.* In Ponape nearly everyone calls an adult man by his political title. The main exceptions are intimate elders and intimate age-mates of the same sex, who may use the man's name or nickname. A wife uses her husband's title. In Truk, nearly everyone calls everyone else by his personal name, often familiarly abbreviated. Children are likely to call their parents and even their chief by personal name or nickname.

It is to be understood that the judgements given above are based on a comparison of the two societies and are certainly not absolute. Thus, with regard to item 1, there are sacred myths and secular tales in both societies, and occasions in both when each type of narrative is appropriate. There are substantial individual variations in both societies, and on most of these points the range of variation might overlap. One might find, for instance, that some Trukese are more compulsive about exact repetition of magical formulae than some Ponapeans, although the reverse would be more usual. But on the whole and comparatively speaking, Ponapeans tend to regard speech as precious, to be doled out in limited quantities, while Trukese seem to value the simple act of being in communication with a fellow human more highly than the content of the communication. Ponapeans feel that careless speech will cause more trouble than no speech; Trukese feel that it is important to remain in contact with others through speech, regardless of what is said.

These attitudes toward speech fit in with the differences in social structure and attitudes toward others previously described. Since Ponapean society is more differentiated, there is in fact more danger that a misunderstanding will be caused by careless speech. Since Trukese society is more homogeneous, speakers are more likely to be understood even if careless. Since Ponapean society is more differentiated, there is a greater chance that beliefs and customs will be questioned by members of one or

another special interest group. Correspondingly there is a greater need to buttress socially important beliefs and customs by rationalizations and sanctions, and a greater need to abstract out from irrelevant details the essential features of beliefs and behavior on which agreement is needed.

The specific way in which world view can have influence on linguistic change is presumably through its effect on attitudes toward speech and language. Attitudes toward speech and the roles of language which are emphasized in a society will affect the predominant stylistic preferences in the speech of members of the society. And given a fairly consistent set of attitudes toward speech over a period of time, the linguistic alternatives which express the preferred attitudes at one period will in time tend to increase in frequency and become obligatory, as suggested for different types of noun phrase construction in Trukese and Ponapean. At the same time, of course, new stylistic alternatives must develop to express the same sort of attitudes. We have here, then, a mechanism, from social structure through world view to speech attitudes and style, for making socio-cultural sense of the direction of linguistic change in particular societies. This constitutes a somewhat watered down and more explicit version of the Sapir-Whorf hypothesis.

Hymes does not discuss syntactic or other linguistic differences between Hidatsa and Crow in his paper. While this is evidently a matter of intention, due to his desire to call attention to the importance of studying cultural differences in the use of language, we gather also that the linguistic differences are minor since the two societies had been separated for only a few centuries. Probably under most circumstances this is not enough time to permit major linguistic changes to take place in response to new differences in the use of language.

In order to bring some more Micronesian data to bear on this question I have made a comparison of noun phrase construction in the languages of Mokil and Ponape. Mokil is a small atoll about 60 miles east of Ponape. Its language has been considered a dialect of Ponapean, although the two are not immediately mutually intelligible. Most Mokilese adults are to some extent bilingual in Ponapean, the men more than the women.

In political structure and in the behavioral aspects of the kinship system, Mokil currently seems to be more different from Ponape than Truk is. Yet the language of Mokil is clearly closer to Ponapean than Trukese, presumably because of greater historical contact between Ponape and Mokil. Truk is about 400 miles to the west of Ponape without intervening populated islands. In the syntactic features discussed above for Truk and Ponape, I can find no clear differences between Mokilese and Ponapean, although some other syntactic and phonological differences can be found.

Mokil is a small island which is occasionally devastated by typhoons. At such times the survivors have traditionally fled to Ponape until their home can support human life again. The most recent major storm was in the late 18th century, according to the ethnohistorical research of Joseph Weckler (1949). Following this, Ponapeans, presumably of high prestige, helped resettle the island. At least some of the pre-

existing dialect differences must have been obliterated in this crisis of depopulation and migration.

The comparison of Mokil and Ponape, as probably of Hidatsa and Crow, suggest that a separation of a few centuries is not long enough to effect major widespread changes in a language. For a study of the effects of social structure and world view on linguistic change, it may be best to pick groups which have been separated, under fairly stable conditions, for the greater part of a millennium or more.

#### DISCUSSION

LABOV: I think that this paper gives us an opportunity to continue the discussion of linguistic relativity in a more constructive direction. In the framework of an abstract discussion, I would be inclined to oppose one thesis of this paper, since I believe that the relationship between linguistic structure and culture is more likely to be arbitrary than iconic. By this I mean that in between the observable social structures and the observable linguistic structures, there lies such a vast number of small, intermediary influences that no simple formal relationship is likely. For example, in American English, we can see social significance in some cases of stress shift. There are a large number of disyllabic verbs which are converted to nouns by a shift of stress from the second syllable to the first, as *to rejéct*, verb, versus *a réject*, noun. Others with the same prefix do not show such a shift, as *to retúrn*, verb, and *a retúrn*, noun. A few are variable, as *to recóil*, verb, and either *recóil* or *récoil* for the noun; or *to recrúit*, verb, and either *recrúit* or *récrut* for the noun. In each case, the more cultivated form of the noun has the stress on the second syllable; the pre-stressed form is characteristic of lower social class usage, or of the military or sports announcers. Parallel cases are found in place names, as *Detróit* versus *Détroit*. But given this general tendency, I don't think that anyone would claim a universal connection between pre-stressing and prestige. In your own study of /-ɪŋ/ and /-in/ (Fischer 1958), you never thought of looking for an iconic connection between the social meaning of this choice, on the one hand, and the opposition of velar vs. apical nasal on the other.

If we set aside for the moment any question of a universal significance for the position of modifiers, and consider the particular relation between the languages and the cultures that you have described, I think you have given us data that are subject to empirical testing. I would like to assume for the moment that the relationship is as you have stated it. I would personally have a great deal of confidence that you are right, since I believe that as an anthropologist you have an insight into the cultural patterns that is not easy to convey to other people in an article of twenty pages or so. By studying the internal differentiation of this feature in Truk and Ponape, I think that the proposition can reach the level of inter-subjective knowledge.

In Table I, you show five columns indicating different possibilities of position of modification. If the process of differentiation of Truk and Ponape continues today, in

the same way as it has in the past, we should be able to use the same scale to measure the process within Truk and Ponapean societies. Now in columns 2, 3, and 4 we have the types that may either precede or follow. Though you don't show variation in lexical items here, it's possible that for some words the amount of fluctuation is greater than with others; some may be 90 per cent one way and others may be 50 per cent in one position, 50 per cent in the other. Let us hope for the purpose of this test that there are a good number of words that fall in the middle range, and therefore allow individual speakers to make expressive choices. Now if you apply the cultural measure that you have been using to a small number of people, say 24 or 30, they should show some variation in cultural traits. Some would fall further along the CONCRETE side of the concrete-vs.-abstract dimension, or further towards the LINEAR pole of the linear-vs.-non-linear dimension. Wouldn't we then expect that there would be a correlation between the individual's cultural traits and his use of post-positions and pre-positions where a choice was open to him? You are in the best position yourself to decide whether such a test would be meaningful.

FISCHER: Yes, I think it would be meaningful, and it ought to work. I think that I might even be able to do it with some of my folk-tale texts, for instance, from different informants. Of course, one problem is that for some of these words you may have to get a tremendous quantity of text to get enough instances to get statistical reliability for an individual. You could elicit these, perhaps, if you were out in the field.

MATHIOT: A question of methodology: how would you go about distinguishing abstract from concrete meaning?

FISCHER: I simply mean by "concrete meaning" one that involves a large number of semantic elements. For instance, the word *lion* would be more concrete than the word *animal*. I define "concrete" as involving the transmission of more items of information than something that is more abstract. These terms are relative or scale terms, with a continuum in between. I do not think that it would be meaningful to say generally that one word is very concrete or very abstract without regard to some context involving comparison.

MATHIOT: How do you recognize in the language what is more abstract?

FISCHER: Essentially, one must understand the general meaning of the lexical elements involved as well as their situational meaning in the particular constructions under consideration. One then compares the meanings in which one is interested for the items of information which are conveyed equally by both and for the items which one conveys and the other does not. If one finds that term A conveys all of the items of information (or 'semantic distinctive features') conveyed by term B plus some others, then term A is the more concrete. To discuss again the particular example that I used, "two long objects": here are cognate words. In a particular context it would often be possible for the speaker in Trukese to infer that the two long objects referred to canoes before the word *canoe* was spoken. This would not be possible in Ponapean in the particular phrase that I used, simply because in Ponapean the word *canoe*

already precedes this. In other words, for the Trukese speaker in that particular instance when he hears the word *ruefôc*, this raises a question in his mind: there are two long objects, what are they? If the conversation has been about canoes, he is likely to think "canoe". If he does, then he is attributing ADDITIONAL meaning to the word *ruefôc* which basically means "two long objects"; that would be its core meaning. He is making this word more concrete in meaning because he is assigning some additional semantic elements to it — whatever semantic elements go together to make up the complex "canoe", in this language. Of course, by adding more items of information to the meaning of a term, one usually reduces the number of objects which will fit the more precise specifications. Some might want to think of this reduction in number of potential referents as a kind of reduction in 'meaning'; but if so, we must distinguish two kinds of meaning, and it is not this latter kind which increases with concreteness.

STOCKWELL: Your paper contains the following statement: "We may summarize the differences between Trukese and Ponapean noun phrases by saying that Ponapean noun phrases are constructed more tightly than the Trukese, where there are differences." But you pointed out that in the case of Ponapean you have a nominalizing particle *me*, whereas in Trukese you must have an intonational difference. I judge this from another sentence in your paper: "In Trukese the relative clause can always constitute a major sentence in itself if lifted out of context and adjusted in intonation." Now the fact that it has to be adjusted in intonation suggests that there is a particular intonation which links it. What I am trying to understand is your statement that one of them is constructed more tightly than the other. I find it quite difficult to put an interpretation on this in view of the fact that clearly, in both languages, you have a highly explicit rule which would assign a certain status to these relative clauses; in the one case it assigns a particular intonation pattern, in the other case it assigns a linking particle *me*. Take English for comparison. Would you argue that "He said he was going" is less tightly constructed than "He said that he was going"? That seems to be what you are arguing here, and if this is true, then I don't know what you are claiming about the difference between the two languages with respect to syntactic tightness.

FISCHER: One of the answers is that intonation as a syntactic device is less reliable sometimes than the use of separate "function words". There can be mistakes in intonation, and hesitations. At times, in fact, when I was recording texts, I was uncertain whether a particular clause should be interpreted as a relative clause or whether it should be interpreted as a sort of interpolated sentence. We can do this in English, too. We can start talking about someone and say, "Joe — he comes from Texas — did so-and-so", and I think it is meaningful to say that the Trukese can do this. I'd say the only case where there is no problem is when there's no hesitation, where there is very fluent speech, and the intonation pattern is normal. This is what you get in specially favorable situations; it's what you get in folk tale texts where the speaker is familiar with the tale. On the other hand, in ordinary conversation, you are likely

to get hesitations; you are likely to get some sort of ambiguity of the intonation pattern which makes this syntactic ambiguity. Of course sometimes people do interrupt sentences that they have started and start a new sentence, and they never finish the first sentence. Another point which might be raised here is that there is a characteristic intonation pattern for relative clauses in both languages. The relative pronoun in Ponapean, while obligatory, could be argued to be superfluous. This redundancy can be taken as a sign of greater concern for attaching the relative clauses tightly in Ponapean.

STOCKWELL: Is there some sort of significant social conclusion or cultural conclusion that you infer from this notion of syntactic tightness?

FISCHER: Yes; I think it is connected with this idea that the Trukese speaker is more ready for interruption by his listener. He doesn't consider it important to push through to the end of his utterance. The readiness for interruption I regard as related to a conviction that the listener is similar to the speaker and is likely to have the same information. This conviction of comparatively greater similarity between speaker and listener in Truk I regard as the effect of the greater homogeneity of society in Truk, especially the weaker development of social class and formal rank there.

SAMARIN: I think that Dr. Fischer will protect himself from some criticism or misunderstanding if he would give a little more information about his field procedures. He has mentioned texts, for example, but are these dictated or tape-recorded? The first would be interrupted and the second uninterrupted discourse. There would be a lot of difference, I should think, in the 'tightness' found in one or the other. Now if we measured syntactic tightness by the places where pause could occur in a stream of speech, it seems to me that we would get different units for extemporaneous, uninterrupted discourse than for deliberate, dictated discourse. The other kind of information which would be helpful is in answer to this question: in what way were the texts edited BEFORE the analysis began? I'm thinking about the 'tidying up' of texts that field linguists generally find necessary as a preliminary to most types of analysis. But implicit in this tidying up is interpretation of what is 'normal' or 'proper' Gbeya or Sango or, presumably, Truk and Ponapean.

FISCHER: The greatest bulk of my material does consist of dictated folktale texts. In these I made an effort to get informants to give me whole sentences, more or less, which I then transcribed, noting pauses, and making some rather crude notations of intonation. I did have a tape recorder which I used some, but I don't have a tremendous number of tapes. And I did also acquire a fair amount of fluency in both languages over a period of about three or four years.

I would agree that it is important to distinguish between phonological and syntactic tightness. This paper is principally concerned with syntactic tightness, and is only concerned with phonological tightness insofar as that is involved with syntax in some way. Statistically speaking, hesitations occur with different frequency and different average length in different syntactic positions in any language, and can thus be used to provide a certain amount of probabilistic information about syntax. However, the

use of hesitations necessarily varies greatly among different speakers and within a single speaker's speech at different times. Therefore, I would expect that other criteria would always be present to indicate 'syntactic tightness', and would usually be more important than hesitations.

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## SELF-ANNULLING PRESTIGE FACTORS AMONG SPEAKERS OF A CREOLE LANGUAGE

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Prestige in bilingual situations has been generally described as a unidirectional force.<sup>1</sup> It is held that that language or form of language which contributes to one's social advance will dominate in any situation. Unfavored languages suffer by losing speakers to a prestige language and by borrowing features from it. Only in an insignificant way have prestige languages been known both as recipients and as donors, e.g. English, which has received some words from Amerindian languages as well as from immigrants to the United States.

The classic picture of such languages in contact is not entirely congruent with the relationship which Sango has to the vernacular languages of the Central African Republic.<sup>2</sup> There are two significant differences: (1) The direction of borrowing, triggered by the prestige value of the Sango language, is not always into the vernacular languages, although much of this occurs; rather, there is borrowing — better, pumping — into Sango. (2) The mechanism of borrowing is also different because of the absence of clear models.

This study describes what happens to a language which has an important prestige function when many of its speakers recognize that their mastery of it is incomplete. In this situation we expect to find people compensating for their ignorance by using words from their own languages.<sup>3</sup> Such straightforward lexical borrowing we do in

<sup>1</sup> Uriel Weinreich's technical definition of "prestige" as "the value of a language in social advance" is adopted in this paper (1953, 79). He rightfully rejects the use of "prestige" to cover such factors as usefulness, emotional significance, and literary-cultural worth. — Paul Friedrich critically read the earlier draft of this paper and made several suggestions about its organization which I gladly used and here acknowledge.

<sup>2</sup> Recent treatments of Sango are Jacquot 1958, 1961a, b; Samarin 1955, 1963, 1964; and Taber, 1962a, b. For a complete bibliography, see Samarin 1955. — The term vernacular is used of all languages indigenous to the area except the lingua franca. (There is a vernacular "dialect cluster" consisting of Sango, Yakoma, and Ngbandi, which must be distinguished from the lingua franca. For the sake of convenience the term Ngbandi serves to identify the cluster.)

<sup>3</sup> Some idea of the nature of the Sango lexicon is obtained from the following figures, based on the corpus used for Samarin 1963. The words identified as Sango include everything which is not French, even a few naturalized Portuguese and English words as well as those from other African languages.

	<i>Types</i>	<i>Tokens</i>	<i>%</i>
Sango	489	33,989	91.3
French	508	2,518	6.8
Proper nouns		710	1.9
Total	997	37,217	100.0



fact find, but what is more important theoretically is that borrowing is from another language or dialect. This is, moreover, something more complicated than need-fulfilling borrowing, for many Sango speakers are found to reject a Sango word if they see that it is the same as their own vernacular one. Since some of these words are widely used, having the status of "standard" forms, the rejection of these words is interesting. This rejection-borrowing pattern turns out, in fact, to be triggered by a conscious adjustment of one's speech to an assumed prestige model of Sango. This, at least, is the theory suggested in this paper.

In the following sections the specific details of the study are presented, and two of its major implications for sociolinguistics are suggested. The abundance of factual data included in this paper is justified by the fact that they contribute to a better understanding of the problem and to the evaluation of the hypothesis here proposed. Since no such study has yet been published on a creole language, these data also serve to document the lexical diversity of one such language.<sup>4</sup>

#### BACKGROUND

The Sango language is the creolized lingua franca of the Central African Republic (formerly known as the territory of Oubangui-Chari of French Equatorial Africa). It merits being characterized as a creole language by the fact that it is a very much simplified form of a vernacular language of the same name, to which it has a relationship somewhat similar to that which Kingwana Swahili bears to vernacular Swahili. It is therefore of a different genre than creole languages such as Sierra Leone Krio (which is intimately related to English) and Haitian Creole (which is related to French), in that it is genetically fully African. It is a lingua franca, furthermore, because it is still used primarily as a second language by most of its speakers, although there is a growing number of children who are learning Sango either as a first language together with the tribal languages of their parents, or as a first language to the exclusion of any tribal language.

With about one million speakers, including those found in both Congo Republics, the Chad, and the Cameroun, Sango ranks among the important languages of Africa. This position does not result from any significant sponsorship by individuals or institutions for whom it had some particular purpose. The French government, in control until independence (August 13, 1960), made no systematic use of it whatsoever; administrators, nonetheless, were rewarded for learning Sango, as for any other vernacular language. There were few that did, however. Furthermore, no indigenous political party made use of it except when expediency demanded; that is, it never became one of the symbols for independence. Only the Christian missions, both

<sup>4</sup> Taber, 1964a, b, attempts to correlate the incidence of French words with certain sociological factors. In general the incidence is lower in the speech of Central African elite than one would expect.

Catholic and Protestant, have used it consistently and intensively.<sup>5</sup> While it is true that the religious education of the missions probably introduced many people to Sango and perhaps contributed in some ways to their competence in using it, it would be foolish to credit the missions for the obvious increase in the use of Sango through the years.

Today, although French is constitutionally the only official language, Sango serves orally and in writing for the popular dissemination of communications of all kinds.<sup>6</sup> Among government officials Sango is used along with French, although on the informal rather than the formal level. Although it has not yet been adopted by the government for use at any stage of public education,<sup>7</sup> it is the only indigenous language to be used on the government-owned Radio Centrafrique.

<sup>5</sup> There are differences, to be sure, between the degree of Catholic and Protestant use, and even between the use of three of the cooperating Protestant missions; but these differences are not relevant to our study.

<sup>6</sup> For example, the recent program to encourage and organize volunteer labor for national economic and social development, i.e. L'Organisation de l'Investissement Humain, has been given the name *Kwa ti Kodro* "village work", which is meant to translate *Travail pour le Pays*.

<sup>7</sup> The following figures, which include all children enrolled in government-approved schools (but do not include a few score in specialized training), are important for giving an indication of the exposure which children have to the French language. Unlike the policy followed in many formerly British-controlled areas, all instruction is officially in French. Teachers are not by any means averse to using Sango, however, and all of them are proficient in it. The school grounds, in fact, are where many village children, by their own admission, first learn Sango. These figures were obtained from an authoritative person in the employ of the Central African government. While they have to my knowledge never been published, they should be considered fairly reliable for the school year 1961-1962. The total population of the C.A.R. is given as being between 1,400,000 and 1,700,000.

(a) Certificat d'études (given on examination to terminate the education of those children who are not qualified to go on for secondary education): 1,272 passed out of 2,777 candidates.

(b) Examen d'entrer à sixième (given to determine the number who are qualified for secondary education):

1961: 390 passed out of 2,190 candidates

1962: 450 passed out of 4,000 candidates

(c) Number of children in primary and secondary schools, accurate only to within 500 because of a discrepancy my source was not able to resolve. The numbers in parentheses are to be read as ordinals, e.g. (1) = first year of school:

(1) Cours préparatoire-1	29,000
(2) Cours préparatoire-2	16,000
(3) Cours élémentaire-1	11,500
(4) Cours élémentaire-2	7,300
(5) Cours moyen-1	6,000
(6) Cours moyen-2	5,000
(7) Collège, sixième année	900
(8) Collège, cinquième année	530
(9) Collège, quatrième année	267
(10) Collège, troisième année	214
(11) Collège, deuxième année	68
(12) Collège, première année	50
(13) Collège, classe terminale	12

(d) Premier baccalauréat: 12 Central Africans passed out of 37 (as compared with 10 out of 20 Europeans).

(e) Deuxième baccalauréat: 6 Central Africans out of 7 (as compared with 6 out of 7 Europeans).

The importance of Sango in central Africa can be accounted for by two factors. The first is the existence of a large number of languages with no really dominant one among them. The second, which is the concern of this paper, is Sango's prestige value. For the thousands of men and women who are ignorant of the elite language, i.e. French, Sango is the only means for upward social mobility.

The Central African Republic, and probably the other areas where Sango is spoken, has a population which is classified in two ways. First, the inhabitants of Bangui, the Republic's capital, set themselves apart from all other people "in the bush" ("up country", etc.) who are known mockingly as *ázò tí ngòndà* "bush people". Within this sweeping characterization they include even the larger towns such as Mbaiki, Bambari, and Bossangoa, whose inhabitants number from 10 to 15 thousand people. In a similar but not so severe way, the town-dwellers consider themselves superior to the village-dwellers of the outlying districts. In both cases, we can speak of a superior urban group opposed to an inferior rural group.

In the second place, it is wealth and political power that mark off one class of people, comprised of government officials and other regularly employed but non-agrarian individuals, from the rest. This is the "upper class" at the top of a triangle which represents the total population; at the apex would be the elite class which is found in so many modern African societies. Below this upper class the population is not distinctly differentiated, and transition from one segment of society to another can be gauged only by amount of education and material prosperity.<sup>8</sup> Accessibility to higher or lower groups in both directions is equally easy. That is, wealth and education do not themselves tend to act as consistent barriers between people, though of course they can be deliberately used as such. A more effective barrier to social intercourse is still geography. There is relatively little travel between the villages and the towns, and most of it is in the direction of the towns; town-dwellers are rarely seen in the villages.<sup>9</sup>

#### DESCRIPTION OF TEST

While in the Central African Republic two years ago to collect a linguistic corpus for a grammar project, I undertook two projects: a pilot sociolinguistic study, and one to test the degree of linguistic variability in the Sango language. The first study involved the use of a questionnaire, devised by myself, to determine such matters as age and sex of speakers, place and manner of the acquisition of Sango, and attitudes to the role

<sup>8</sup> Some appreciation of the economic level of the C.A.R. is gained from the following facts. According to the Association Internationale de Développement (as reported in *Le Moniteur Africain*, Dakar, 25 Janvier 1964, p. 7) the gross national product in 1961 was 12 billion francs C.F.A. or about 49 million dollars. In this same year the average individual income was less than \$30.00. Among the 35 African nations for which there was information, the C.A.R. ranked 34th and 31st in these two categories.

<sup>9</sup> An indication of the limited travel to the capital is seen in the data on the informants. Of the 27, only 12 have ever been in Bangui, of whom again only 10 were there for more than a few weeks.

and function of French, Sango, and vernacular languages. A third-year "collégien" assisted me by interviewing at random; my own use of the questionnaire was in studying a small Gbeya village in depth.<sup>10</sup>

It is not this proper sociolinguistic survey, however, which led to the formulations proposed in this paper. Data of sociological significance came from an unexpected source, i.e. the second survey mentioned above to test linguistic diversity. This included both a phonological and a lexical check-list, the purpose being to measure and describe the diversity observed in Sango. Since Sango is a second language for most of its speakers, the survey naturally revealed variations of the type that have led many to charge Sango with having practically no national homogeneity whatsoever.<sup>11</sup>

The lexical test consisted of asking informants for the Sango names of 24 food products, most of which were widely used in the Central African Republic. The test items were for the most part chosen at the open market in Bangui, the capital of the country. Grains and small dried fruits and leaves were preferred for convenience of handling; they were put in clear-glass pharmaceutical bottles about 2½ inches in height. A few fresh leaves were used under plastic covers until they became too discolored for easy identification. Food products were chosen for the test because this lexical domain was known to reveal considerable diversity. Precisely what items revealed the greatest diversity was not, however, known at the time. The selection might therefore be considered random with the qualification that I tried to make certain that only food items characteristic of the indigenous cuisine be included in the test: e.g. *kəkə* (number 34) is never eaten by Europeans.

The interviews were conducted in the Sango language, by myself. Only one question

<sup>10</sup> This study was done in connection with a six-week period of fieldwork undertaken in July and August of 1962. It gives me pleasure to acknowledge once again the assistance, financial and otherwise, of the following agencies who made my work possible: the American Council of Learned Societies, the Foreign Missionary Society of the Brethren Church, the Sango Grammar Project (with a grant from the Department of Health, Education, and Welfare through NDEA, Title VI), and missionaries too numerous to mention here. Previous to 1962 I was a resident of the country, at Bozoum, Mbaiki, and Bossangoa, for seven years. The questionnaire was drawn up after consulting the one used by Rubin, 1962.

<sup>11</sup> The views expressed by a linguist are as unexpected as those by laymen are expected: Jacquot, after a two-month period of fieldwork, limited for the most part to Bangui, can claim (on what empirical basis?) that "Le sango n'est pas une langue homogène..." (1961b, 173). In an earlier report his statement was even stronger: "La diversité des influences subies par cette langue fait qu'elle n'a qu'une unité apparente et qu'il serait plus juste de parler de dialectes sango que de la langue sango" (1959, 5). No one can deny linguistic diversity in any speech community — even French, whether it be of France, Belgium, or Canada. It is the nature, degree and sociological correlates of this diversity that must be described. Remarkable homogeneity has been discovered in the language (Samarin, 1963); what differences there are — phonological and lexical — leave no room for its being reduced to an aggregate of "dialectes". Jacquot's observations may be criticized on several other points. For example, without having undertaken any extensive survey, he can yet say that "sa [Sango's] connaissance et sa pratique ne sont pas générales..." (1961b, 173). While the word "générale" gives little statistical information, its implication is clear enough. My own study of three remote villages in the Bozoum-Bossebele-Bossangoa areas (1962, not yet published) showed that Sango was so well known that almost all the women — Protestant, Catholic, traditionalist, young and old — had a fairly good knowledge of the language.

was posed to the informant — and it was rare that it had to be repeated after he knew what was requested — sometimes in the form *éré tí só nà yángá tí Sango yè* “What’s the name of this in the Sango language?” or *mò dí éré tí só nà yángá tí Sango, mò tènè yè* “What do you call this in the Sango language?”. Great precautions were taken at the beginning to distinguish between the Sango language<sup>12</sup> and the informant’s native language; but once he began to name the items, he was not interrupted, even if he obviously was mixing the two. Circumstances — like an unsolicited comment by a bystander — sometimes led, however, to obtaining both the Sango and vernacular forms for a single item. Otherwise, elicitation of the vernacular list followed the Sango one.

The interviews were without exception relaxed events. The informants did not give the impression of being tested. Since I had told them that I was studying the Sango language (*mbi yí tí híngà yángá tí Sango nzóní* “I want to know the Sango language well”), the informants behaved as if they were helping me.

#### RESULTS OF TESTS

The detailed results of the test are given in Appendix II. In this section I shall deal only with the data germane to my principal subject.

The most significant aspect of this study concerns a result which was not at all expected: namely, that several informants, in apparent ignorance of the common Sango words for some of the test items, used words from other dialects of their own languages or from completely different languages.<sup>13</sup> Although such responses were few in number, in comparison with the general pattern to be discussed shortly (i.e. of Sango known, Sango unknown but vernacular known, Sango confused with vernacular, or item not known at all), they are striking. A theory must explain their occurrence, and

<sup>12</sup> In this study the real etymology of a word is of consequence only because we are interested in determining the difference between vernacular and Sango words. The fact that a particular word is ORIGINALLY from Banda, Zande, etc., is in itself unimportant. Information concerning source of borrowing is, of course, valuable for other kinds of studies.

<sup>13</sup> It is possible, of course, for me to say only that at the time of the interview a word was USED, not INTRODUCED for the time in Sango. Except where an informant volunteered the information that he or other people used another word also, I can only assume that the word given was unique in his speech. This is not necessarily true, however, as any experienced field-worker will readily admit; only when an interview is carefully controlled, when data are abundant, and when possible errors are factored out, can one begin to feel confident of his data. Of one thing I am certain: in no case did an informant give the impression of having been induced into giving a word by the artificiality of the interviewing situation. Such spur-of-the-moment borrowings would obviously skew the data to the point of making my hypothesis extremely doubtful. It might have been valuable to have learned from the informants if they realized that they were using words from other dialects and languages. In an attempt to acquire some such information, I sent out in January 1964 a questionnaire with 42 words identified as Sango or vernacular by the informants I used in 1962. The 36 European correspondents were requested to interview Central Africans with the purpose of learning from them what they thought was the language source of each word in the list. Data obtained from 17 individuals in this way does not reveal any patterns of sufficient interest to warrant being reported here.

this theory must take into account certain sociological factors, concerning both the country as a whole and the informants in particular. I account for these instances of borrowing by the relative prestige value of Sango as opposed to the vernacular languages. My hypothesis is that when a speaker is aware of several different words for certain objects or concepts, he will reject those that are most like the ones in his own language, being guided by the feeling that they are inferior to Sango. In some instances a word from his language may have been completely naturalized in the Sango language and be in general use throughout the country, but its identification with his vernacular is reason enough for him to reject it. Instances of this kind of borrowing-rejecting pattern are given below under *Corroborative Evidence*. Before the data are presented, however, we need to see three other facts which emerged in the testing, none of which is unusual in a lingua-franca of this type.

1) The names of some test-items were unknown. This ignorance is correlated in part with inexperience with the world outside the local rural area. This would mean that an untraveled person would tend to know his own language's word for an item but not the Sango one, even though that item is well-known throughout the country. Even if he had occasion to buy some of these products at the town market nearest his home village, he would usually be buying from a person speaking his own language.<sup>14</sup> The reason is that most of the towns are in areas where one language predominates.<sup>15</sup>

2) Several informants gave words from their own languages as Sango. This did not result from a memory lapse in every case; some informants insisted that the word they gave was both Sango and vernacular. Such borrowing is no different from the use of *séti* and *mbásámbará* for "seven" by some speakers of Sango who do not know the French source for *séti*, which is clearly *sept*. The effect such lexical interference and borrowing has on Sango is obvious and needs no comment here.

3) A few test-items were not recognized at all; but apart from reducing the amount of information, this fact does not in any way skew the results.

<sup>14</sup> One must understand that these markets are not at all comparable with those in much of West Africa. There, the market complex includes regular market-days and rather extensive travel to the market sites by sellers and buyers. In the C.A.R. the markets exist almost exclusively in the administrative centers, where the préfet or sous-préfet now resides, to provide food for the resident population and the transients.

<sup>15</sup> The only population statistics available to me come from a report submitted to the Bureau d'Études et Recherches du Plan, entitled "Mission Centre Oubangui: Recensement des Centres Urbains de la Kémo-Gribingui" (date unknown). It dealt in 43 pages (duplicated) with the towns of Fort Sibut, Fort Crampel, and Dekoa, as follows:

Fort Sibut: total population, 7,341. Banda: 46 percent; Manza: 46 percent (but this proportion of Manzas seems to be recent, because they comprise 70% of the total population who have arrived since 1950); Other: 8 percent.

Dekoa: total population, 3,118. Manza: 69 percent; Banda: 20 percent; Other: 11 percent.

Fort Crampel: total population, 7,814. Manza: 64 percent; Banda: 22 percent; Other: 14 percent.

The tribal mixture in these towns may be higher than in many other areas, however, since (1) they are on one of the major roads which lead to the Chad, and (2) they are located in the area where two principal groups, the Banda and Manza, meet.

## INFORMANTS

No difficulties were encountered in the selection of the informants. Since my principal activity was the recording of Sango speech with a tape-recorder, I had only to open my test-box to attract intense interest while I gave my (divided) attention to the recording. Long narrations gave me ample time to observe the audience and choose my informant. This was done with a deliberate plan in mind: it was important to the study to obtain maximum diversity, such as might approach the state attributed to Sango by uncritical observers. For this purpose I needed people whose experience with Sango was limited to a local variety. I imagined these to be either people who had never worked under polyglot situations (e.g. in the administrative centers, at commercial establishments, etc.) or those who had not traveled extensively. This would mean the young and the old, women rather than men, the unschooled, and the traditionalist rather than the Christianized. As it turned out, however, a few of my informants were quite sophisticated, although they did not represent the elite stratum in the society. Finally, I needed a good cross-section of the country, geographically as well as linguistically. These criteria were important, but their application at any place was influenced by circumstances beyond my control.

Among the several informants, two deserve special mention because their responses are typical of the general kind being described here. The other informants are more simply characterized in Appendix I.

Banda informant 15 is typical of the rural Central Africans who are consciously trying to identify themselves with the society and culture lying beyond the limits of their villages and kinship circles. He was uncomfortable with his backwardness, it seemed to me, and was eager to improve his position in life. Already in his early twenties, he had never gone to school and had never worked for a European, even though he had lived in Bangui for six months. He was quite explicit about the value of Sango, saying that parents taught their children Sango in the villages because without Sango one could not get jobs. He admitted that the Sango of Bangui was different, but that when he lived there, he used to ask people to help him improve his own Sango. For this young man, ignorant of French, Sango was clearly the only means for improving his station in life. This characterization finds clear corroboration by the young man's rejection of Banda words, even the widely-used ones, for words from other languages in his Sango.

The other informant (no. 27) also frequently adopts words from another language, but he is less of a model than the young man above. This is the Isungu *chef de quartier* at Berbarati, who claimed to have lived in that town for the last 30 years. Although an important figure in the Isungu community, he undoubtedly was a bilingual in the local Gbaya dialect. In any case, it is from this dialect that he draws his "Sango" words. The motives for doing so are not clearly the same as for the young man just discussed; if they are incontrovertibly different, he presents us with a case of language interference in a third language, i.e. from one's second instead of first language.

## CORROBORATIVE EVIDENCE

Of the 19 test-items which revealed lexical variation, 9 of them are characterized by the type of borrowing described above. They constitute in my opinion corroboration of the theory here being suggested. Test item 5 is discussed in some detail; the rest are treated in summary fashion. This item is an oily seed (*Citrullus vulgaris* Schrad.) which is widely used in the preparation of a common food.<sup>16</sup> It was identified by no less than 7 different words, and 9 informants claimed that they did not know what its proper Sango name was (see Appendix II). But this information does not have the significance that the following does: consider the fact that there are three Banda informants (nos. 9, 15, 16) and one Gbaya informant (18). Consider also the fact that two informants claimed not to know the Sango name for the grains and that the others used either a Banda word (*mbákéré*) or two words of undetermined origin (*séré*, *kpt*). From these facts we might infer that the Banda speakers used the Banda word *mbákéré*, and that it was the Gbaya who could not identify its Sango name. Rather than this, however, we find that the Gbaya (18) informant was the one who used the Banda word; whereas two of the Banda (9 and 15) professed ignorance of the Sango name, offering the information that *mbákéré* was a word from their own language. The third Banda, however, attributed *mbákéré* to the Gbaya! He used only *séré* and *kpt* for this grain. Presumably he would ascribe the presence of *mbákéré* in his own Banda dialect to borrowing from Sango. In this instance, therefore, we clearly see the rejection of forms from one's own vernacular, the adoption of forms from some other language, or the indecision about what are the authentic Sango forms. Of course, this was indecision only at the time of the test; in some contexts these speakers might very well use their Banda word while talking Sango; or having heard another word in another context they might use it, perhaps only for that occasion. The remaining eight test-items reveal the same kind of rejection-adoption pattern.

Item 3: Corroborative evidence is furnished by the word *zùrù*, which is widely used and commonly recognized as coming from the Banda language. Two sets of informants provide the data: (a) Isungu informant 27 used the word *zùrù* in his own language even while recognizing its source, but the word *pón*, from the local Gbaya, in Sango. (b) Of the eight Banda informants, only one, no. 20, used *zùrù*; the others used *bondo*, the etymology of which has not been traced. I assume that at least some of these seven informants have rejected their own vernacular *zùrù*, not realizing its wide usage; in the absence of their vernacular forms, the evidence remains assumptive.

Item 4: Three different informants provide corroborative evidence with respect to this uncultivated food item.

21 (Gbaya/Bogongo): *ngálá* < Gbaya/Berberati

26 (Gbaya/Yangere): *ngíri* < Banda

<sup>16</sup> Italicized numbers refer to the test items; the other numbers refer to the informants. In this discussion words are identified by only one of their variants. The full range is indicated in Appendix II. The sign < indicates origin. Botanical identifications were made with a judicious use of Tisserant.



27 (Isungu): *ngálá* < Gbaya/Berberati

It is extremely important that informant 26 uses a LOCAL Banda word; if he had used one from the east (e.g. *ngirikí*), we would have to admit that he had simply learned a generalized Sango word.

Item 6: The best corroboration is provided here by informants 13 and 21.

13 (Banda): *sóngbá* < Banda?

21 (Gbaya/Bogongo): *sùndù* < Gbaya/Berberati

The Banda informant's response is significant only when it is pointed out that she considered *sindì* (the common word for this grain) to be a Banda word, although it is not Banda at all and is probably the most common Sango word for sesame.

Item 8: Once again informants 13, 15, and 21 provide corroborating evidence.

13 (Banda): *kákéré* < Banda

15 (Banda): *dàùdá* < ?

21 (Gbaya/Bogongo): *gónò* < Gbaya/Berberati

Both 13 and 15 are acquainted with *kákéré* and *dàùdá*, but they use them differently: the first claims that *dàùdá* is Arabic and the second that *kákéré* is his own vernacular.

Items 9 and 10: Isungu informant 27 again adopts the Gbaya/Berberati words. For 9, Banda informant 15's use of *kpl* is possibly significant, because he rejects his own *kóssò* which elsewhere is one of the proper terms for this seed.

Item 21: Banda informant 15 once again makes a significant switch. In spite of the fact that some form of the Banda *vèkè* is almost universally used in Sango (e.g. by 25 out of the 27 informants), this young man rejected it (though fully recognizing it as his own vernacular form) for *dàràbà*, which is unique in the study.

Item 22: In spite of the extreme diversity in the identification of these leaves, something of significance is to be found in the distribution of three vernacular words used by "outsiders" as Sango. (a) Gbanu informant 22 considered *ngbùrú* to be Sango, for she gave *gbààrò* for her own vernacular; but Gbaya informant 3, interviewed at Bouca, gave *ngbùlú*, clearly the same word, as his own, and did not know the Sango. (b) Kaba informant 23 did the same thing, taking *bàlò* from some as yet undetermined form of Gbaya speech, modifying it in the process (say, from *gbààrò*) or taking it in an already modified form. (c) The form *bùrúgùtù*, used by Isungu informant 27, is obviously taken from a Gbaya dialect, for informants 21 and 26 give some form of *gbùrúgbùtù* as their vernacular.

#### SOCIOLINGUISTIC IMPLICATIONS

The phenomena which have been observed in the use of Sango by Central Africans are not without significance to our understanding of two subjects frequently mentioned in connection with sociolinguistic studies, namely, the role of prestige in language usage and the processes leading to language uniformity and diversity.

The conclusion that one must draw from these Sango data, sparse though they may be, is that a certain amount of lexical variation in Sango must be attributed to the

opposition of prestige and non-prestige values in the society. Here, then, prestige works contrary to what one expects of it. Rather than being a force which leads people to UNIFORMITY in speech, it leads them to HETEROGENEITY. Wanting to speak proper Sango, people reject whatever they think is local; proper Sango is simply that form of the language which does not betray their origin. It is as if speakers sought for signs of emergence from the rural scene, since the village-dweller was considered as having nothing and knowing nothing; he was a non-entity.

#### ROLE OF PRESTIGE

The identification of prestige with the language itself is in negative terms. It is certainly not in terms of any particular dialect. While Central Africans can very often identify the ethnic or linguistic background of a person speaking Sango, they do not seem to recognize any other substantial patterned variations in the language. This is not from pride, nor out of defense for the universal integrity of the language, as seems to be true of speakers of Paraguayan Guaraní.<sup>17</sup> The most characteristic attitude of Sango speakers is in fact one of tolerance. On several occasions when I have asked individuals about their evaluation of Sango speech which on linguistic grounds was definitely eccentric, whether spoken by Europeans or Africans, they have always expressed no cognizance of the differences but rather various degrees of surprise at my own observations. Excluded here, of course, is the speech of people still learning the language. Of them it would simply be said *álà dè àhngà Sango kóé àpè* "They don't yet know all of Sango"; this is meant as a simple statement of fact, not of attitude.<sup>18</sup>

By saying that language-linked prestige is negatively ascribed, I mean that that form of Sango which is least localizable is most acceptable. This appears to be as true for phonological aspects of the language (as when a Kaba uses *f* for *p*) as for the lexical, although phonological differences seem to be better tolerated than are lexical ones.

It would be wrong, however, to interpret these statements as constituting a role of

<sup>17</sup> Garvin and Mathiot 1960, 786. The Sango situation is also unlike the Jamaican one, where intolerance is articulated in terms of explicit evaluations. De Camp writes: "When I have suggested to informants lexical variants characteristic of another area, I have seldom noticed any reaction other than amusement at such absurd and outlandish words" (1961, 74).

<sup>18</sup> It should be of some interest to note that I have never found an incipient Sango bilingual whose speech was halting or ungrammatical. Speakers may be embarrassed at speaking it (like adolescent Gbeya girls in the villages) or even deny any knowledge, but in every instance where speech duration was long enough for some observation (and I have a few tape-recordings), I found a typical Sango structure. Women are sometimes attributed a better knowledge of Sango than men, especially in Bangui. Jacquot reports: "Les hommes leur reconnaissent souvent une certaine supériorité dans la connaissance et la pratique du sango, car, disent-ils, elles sont beaucoup plus bavardes qu'eux" (1961, 174). This fanciful opinion hardly bears repeating as a source of information about language competence, although it can be regarded as linguistic pseudo-evidence for certain behavior. When analyzed, the Sango of women was no different from that of men (Samarin, 1963), but men have expressed their pleasure in the voice qualities of women's speech. It is smooth, oily, etc.: *ànzèrè mingi* "it is very sweet".

evaluation used by the superior strata of society, for even here there is great tolerance. Recognized localisms are interpreted as deviations from the assumed norm only under certain conditions which have not yet been adequately studied.

But we are not so much concerned with potential confrontations of various forms of Sango as we are with the more powerful force of attitude in each Sango speaker. There is an abundance of NON-LINGUISTIC evidence, certainly, for believing that Central Africans want to lose their village identity. What this study has revealed is a peculiar type of LINGUISTIC evidence for this belief, namely, that the speaker in many instances will metaphorically grab at the closest available word, EXCEPT HIS OWN, when he wants to use proper Sango.

The Sango prestige configuration, while being idiosyncratic in some of its details, is not without parallel elsewhere. The most obvious one is the relation of non- or sub-standard idioms to standard languages. Here too speakers of the non-standard forms consciously seek to shed their localisms, leading to hypercorrections, a phenomenon which is documented with great skill in Dr. Labov's paper at this conference.

What is significant about the Sango case is that it validates an assumption made by Fischer in connection with language change. He suggests that "all societies possess some form of elite group — if only the 'ideal conformist' in some societies..." (1958, 53, fn.). In the Central African Republic this group is clearly comprised of all those people who seek to shed their localisms. It is an "ideal" group because it has no geographical locality; it is still a model in the making.

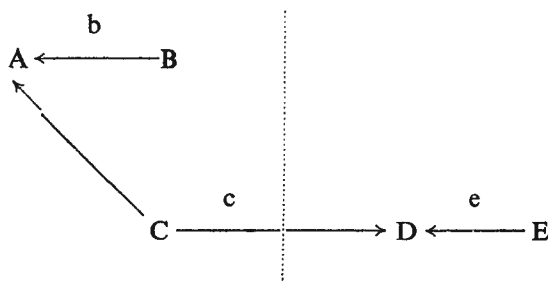
#### LINGUISTIC DIVERSITY

While it is quite clear that the force of prestige is at work among speakers of Sango, it is not so clear that its linguistic effects are always those one might expect. Rather than standardization and uniformity,<sup>19</sup> the data seem to indicate that it is increased variation which conformism leads to. In the absence of a clear model, a speaker's predisposition to conformism is inadequately realized; it is in fact unrealizable. It nonetheless produces a linguistic event which somehow satisfies the speaker's sensitivities. When speakers A and B each use the other's vernacular forms (*b* and *a* respectively) for Sango, they obviously have not really measured up to the norm but only THINK they have. The result of such prestige-satisfying efforts results in variants *a*, *b*, *c*, *d* ... *n* for any concept. (That the variants are in fact limited is immaterial here; we are not presently interested in a measure of the variability within Sango, but in its causes.)

The pattern just described can be illustrated by the following diagram, where arrows point away from the donor to the recipient languages. Proximity represents

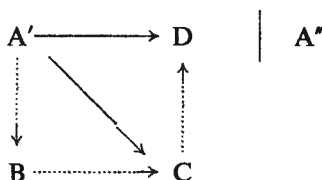
<sup>19</sup> For example, Lunt seems to suggest (1959, 23) that the strong tendency of Yugoslav villagers to adapt their own speech to fit the circumstances led to interdialects, nowhere defined, which constitute the base of Standard Macedonian. He writes, "I strongly suspect that the agreement [on what was acceptable Macedonian] stems in part from the existence of two or three 'interdialects', local koinés used in the larger market-centers by peasants from outlying villages. I was struck by the chameleon-like adaptation of the speech of unschooled villagers whose dialects I wanted to record..."

degree of relatedness with the broken vertical line dividing the most remotely related or non-related languages:



Thus, there are three donor languages, B, C, and E, giving rise to three different but synonymous words, *b*, *c*, and *e*. The diagram is incomplete because it does not show that some speakers of a language may borrow from another language which itself receives words from their own, e.g.  $A \leftrightarrow C$ . Such is the case when a Banda speaker uses a Gbaya word and a Gbaya speaker uses a Banda word. Although my study does not show much evidence of this two-way borrowing for specific items, it can be assumed that it goes on for Sango in a general way. It can also be assumed that some languages or dialects, such as A and D, are not so much donors as they are recipients. This fact would be represented by several numbered arrows directed from, say, C to D. To some degree this can be documented for Sango: more languages seem to receive FROM Banda than give TO Banda. But it is possible that this is a flow which can only be looked at diachronically; there is less evidence for such flow today than there was in the past, when Banda men served in the French colonial militia.

It should be carefully noted that the preceding discussion deals only with the INTRODUCTION of new words into Sango, not of their spread. On the latter subject this essay has been discreetly silent. The distinction, of course, is a necessary one. Though a person may be strongly motivated to use a word from language B, this fact in itself is no guarantee that others will imitate him. The factors that may hinder or encourage the spread of an innovation will not be suggested in the present study, except for the observation that my theory assumes the rejection of some words by some individuals because of identification with words in their own languages. This rejection can be illustrated by the following diagram:



That is, a word may be borrowed from A', either directly (solid arrows) or indirectly (broken arrows) into B, C, D, but not into A'', the reason being that A' and A'' are

dialects of the same language, or closely related languages, whose words for the object in question are identical. This word may be a very common one in the lingua franca because speakers of B, C, D, etc. use it; but since the speaker of A recognizes it as one of his own, he rejects it.

The process being postulated in this study is shattering to the lexicon — if one looks at it statically. It would be more accurate to use a dynamic metaphor and say that the process produces more lexical targets or models than are useful or necessary. It is for this reason that it is described in the title of this paper as “self-annulling”: i.e., prestige perpetuates localisms rather than creating a standard form of the language.

It is difficult to ascertain the spread of the phenomenon described in this paper. Any parallel case would have to have two major characteristics: (1) a prestige model or ideal, if only negatively defined; and (2) speech diversity or indeterminacy which results from the response of speakers to the prestige ideal. A clear understanding of this second characteristic is needed to avoid making false analogies. It must be remembered that we are not concerned with all those individual cases of language “improvement” among people who are conscious of an inferior status, real or imagined. Closer to our study are instances when individuals choose to follow wrong models, leading, for example, to *aren't I?* instead of *ain't I?* and *to you and I* instead of *to you and me*. A whole speech community must show similar tendencies.<sup>20</sup>

Two such apparent cases are the following. The first involves idiolectal speech variation among rural Englishmen:

Anyone with direct knowledge of current English vernacular will know that dialect speakers are quite inconsistent in their pronunciation. So strong is the pressure of the standard language upon them, so willing are they to accept variant forms, and so ready are they to modify their own pronunciation in conversation not only with people outside their own intimate circle that their spoken words often occur in three or four different forms. (Orton, 1962, 18-19)

The second involves certain forms of extreme language urbanization. One of the best described cases is that of Town Bemba, a language of the “Copperbelt” of Northern Rhodesia. The language is a form of CiBemba which seems to have no uniform linguistic structure. It is not, in other words, a patterned dialect parallel in some way to vernacular Bemba. Rather it is characterized by “the somewhat negative tendency to indeterminacy in most departments of the language”.<sup>21</sup> More specifically, it “has no real homogeneity but is merely a complex of different ways of speaking an urban lingua

<sup>20</sup> If analogical overcorrection, such as hyperurbanisms, could be shown to have skewed some linguistic patterns on a rather extensive scale, it would then be admissible here. But such cases are unlikely since analogy, by nature, works toward uniformity.

<sup>21</sup> Richardson 1963: 145. The implications of Town Bemba for the explanation of the origins of pidgin languages are not made by Richardson, but these are hardly avoidable. The theory most commonly held is that of inadequate learning: i.e. when a group of people inadequately learn a target language, the result is a pidginized form of it. (Considerable progress has recently been made in accounting for creoles such as English, Spanish, French, and others by tracing them to a widely used Pidgin Portuguese.) That this theory does not account for Sango has already been pointed out by me; that it does not account for Town Bemba is equally clear. Yet Town Bemba must certainly be considered a kind of pidgin language.

franca based on Cibemba". Those statements account for the linguistic factors. The attitudinal factors are summarized in the following: "To a certain extent TB [Town Bemba] is a political symbol, a conscious reaction against the old way of life..." The prestige value of Town Bemba is clearly inferrable from that statement.

It would be surprising indeed to find that the phenomenon described in this paper has occurred only two or three times. Other languages marked by extensive lexical borrowing might upon further study prove to have been subjected to similar prestige forces. For the evidence to be valid, however, it will have to be shown that the lexical diversity did not simply result from prolonged exposure to other idioms. All languages can and do borrow — which is of course only a metaphoric way of saying that their *SPEAKERS* borrow — throughout their existence, but not all languages have to bear the stress and strain of man's will to improve his station in life.

It is for this reason that the history of Ashkenazic Yiddish is so intriguing (Herzog 1964). Though its lexicon is replete with words from Hebrew-Aramaic and from various German and Slavic dialects, it can not YET be shown that this mixture is due to strong prestige forces. While it is true that different dialects are characterized by having certain objects or notions represented by words from different languages or dialects, it is also true (and more interesting) that the words of a particular dialect for such pairs as *button* and *to button up*, *to bless* and *blessing*, *to curse* and *cursing* do not come from a single source as one might expect. For some reason not yet determined, the words of each pair have their own histories.

There is of course the strong possibility that the phenomenon we observe in Sango and Town Bemba is characteristic only of languages which have gone through a period of pidginization. Since several of these languages are still in existence and are, moreover, used in societies where language is an explicit status-ranking symbol, we should hope that investigators in the future will devote time to the search of similar prestige-triggered lexical diversity. Should they produce other such cases, we would have one more feature by which to characterize the creole languages — one which closely links sociological facts to linguistic ones. Having done this, our next step might not be entirely in vain: that is, the study of similar phenomena in the traditional "normal" languages.

#### APPENDIX I: INFORMANTS

The informants used for this survey are described below. The following abbreviations are used in the identification:

- gb Gbaya-Manza language family
- b Banda language family
- s Sara language family
- y Yakoma-Sango-Ngbandi language family
- i Isungu (also known as Mbati)
- f female
- m male

r	reads
w	writes
/s	Sango
/f	French
P	Protestant
RC	Catholic
illit.	illiterate

The entries, following the identification of sex, appear in the following order: native language, place of birth or long residence, literacy, religion, residence in Bangui (identified by the name of that city). The absence of an entry indicates that no information was obtained. Of the five language families represented above, all but Sara and Isungu are Adamawa-Eastern. Sara is Central Sudanic (of Chari-Nile) and Isungu is Benue-Congo (i.e. Bantu). Native languages are specified whenever possible by a "tribal" or geographical term, e.g. Gbaya/Berberati.

- 1gb f, Manza, Bouca, rw/s, P.
- 2gb f, Manza, Bandoro, illit., P.
- 3gb m, Gbeya, Doungbou, r/s, P, Bangui.
- 4gb m, Manza, Bouca, r/s, P.
- 5gb m, Gbaya, Kamakota, r/s, P, Bangui (often).
- 6gb f, Manza, Dekoa, r/s, Bangui.
- 7b m, Banda/Dakpa, Banguila, illit., Bangui.
- 8b m, Banda, Ndele, r/s. Unmarried, young man, works in the mines at Bria.
- 9b m, Banda, Ouada, r/f, Bangui. Graduated from the Centre de Formation Professionnelle in Bangui around 1959. His Sango speech revealed much interference from Banda, e.g. the palatalization of /ti/ to [či.]
- 10b m, Banda, Ippy, r/f. About 13 years of age.
- 11s m, Sara-Madjingay, Nara (Chad Republic), r/s. Learned Sango after becoming a Christian, when he was already a married man.
- 12y m, Mbangi, Alindao, rw/s, f, P, Bangui. Had two years of elementary education in French. Worked at mission hospital from 1957-62. During this same period he served as an informant on the Sango Bible Translation Committee.
- 13b f, Banda, Alindao, rw/f, RC. This young girl was enrolled at the RC school at Ft. Sibut, Cours Moyen (first year).
- 14b m, Banda/Djoto, Makorou, illit., RC. This middle-aged man had never worked for a white man. During the interview bystanders made fun of him for not knowing Sango well. Data obtained from him are not reliable because his Sango and vernacular lists are identical.
- 15b m, Banda/Djoto, Yatiganza, illit., Bangui (six months). A young man in early twenties who had never worked for a white man. He claimed to have learned Sango in the village.
- 16y m, Yakoma, Kalimonga (near Ouango), rw/f, Bangui (seven months). Had seven years of education in French. Employed at Bambari as a gas-station attendant.
- 17gb m, Manza, Dekoa, r/f, s, Bangui (three years). Had four years of education in French.
- 18gb m, Gbaya, Cameroun, Bangui (two weeks). Interviewed at Berberati.
- 19gb m, Bogoto, Carnot. Had three years of education in French. Now employed as house-servant.
- 20b m, Yangere, Berberati, r/s, P. Has traveled a lot because of being employed as a truck-driver, but he has spent only a few days in Bangui.
- 21gb m, Bogongo, Gbalondo (near Nola), Bangui (several months).

- 22gb f, Gbanu, Bounguinza. An older adult.  
 23s f, Kaba, sous-préfecture of Paoua, r/s, P. Had been in a multi-lingual environment for three years because her husband was a student at a Bible institute near Bozoum.  
 24gb m, Gbeya, Bossangoa, r/s, P. Employed as a tailor's assistant in Bossangoa.  
 25gb f, Gbanu, Bokoin, r/s, P. The interview was done at Bossembele where she and her husband were attending a mission Bible school. She had never before resided outside her own village.  
 26gb m, Gbaya/Yangere, Nagate (near Berberati). A young man whose manner indicated that he had not had much experience outside his own village. He did not seem to know Sango well.  
 27i m, Isungu, Mbaiki. An elderly man, chief of the Isungu section of Berberati where he had lived for 30 years (or so he claimed).

#### APPENDIX II: DESCRIPTION OF TEST-ITEMS

Only the words given by informants as Sango are cited; their own vernacular forms are not listed here. The origin of the word, wherever known or strongly suspected, is enclosed in brackets []. Informants who used each term are identified by their numbers in parentheses. Also indicated are those informants who did not know the Sango name for the test-item or did not recognize it. Some items were not tested at all, either by oversight, misplacing of the item, etc.

2, dried corn kernels: *gbànzà* [?] (2, 3, 5, 8, 14, 18-22, 24, 26), *nzo* [?] (by all others). The closest parallel to either one of these words is *gbonjo* (Banda/Togbo) which Tisserant identifies as a white-grain corn "à épis longs et réguliers."

3, dried grains of mil (*Sorghum guineense* Stapf.): *bondo* [?] (1-9, 12, 13, 15-17, 24), *pón* [Gbaya] (21, 27), Sango unknown (25), not identified (26), *zùrù* [Banda] (by all others).

4, grains of an uncultivated tree (*Beilschmidia ngriki* A. Chev., "Arbre de galeries commun.... La graine préparée est un condiment mucilagineux..."): *ngéréké* [Banda] (1-6, 25, 26), *dngó* (19, 22, 23), *dngóló* (7), *dngó yàngóró* (10, 12), *ngálá* [Gbaya] (20, 21, 27), *piè* [Banda] (8, 9, 13-15), Sango unknown (11, 17, 18, 24). The words *dngó*, *dngóló*, and *dngó yàngóró* are probably generic terms for mucilaginous foods, for one can use the expression *dngó dngó* even of slippery ground: cf. 21 and 22. According to Tisserant, *piè* is not the same as *ngéréké*, but *Beilschmidia piya* A. Chev. I have no way of determining now if the informants who used *piè* would distinguish between that and *ngéréké*, either in their own language or in Sango.

5, oily seed (*Citrullus vulgaris* Schrad., cultivated for its edible seed): *kóso* [Banda?] (20, 21, 25-27), *kúkúru* [?] (5, 23), *mbákéré* [Banda] (7, 8, 10, 14, 16, 18), *mbéréké* [Gbaya] (1, 6, 19, 22, 25), *séré* [?] (13, 16), *kpi* [?] (16), *bákpá* [?] Sango unknown (2-4, 9, 11, 15, 17, 24). Both *kóso* and *kpi* are also used of 9. Perhaps *kóso* is used by some people to refer to any edible oily seed. The word *kpi* may also have some generic meaning, for it is also used of any oily edible paste, e.g. crushed sesame or peanut grains. The similarity between Gbaya *mbéréké* and Banda *mbákéré* (especially the variant form *mbékélé*) is



striking; there seems to be a metathesis of the last two syllables. It is doubtful, however, that these are synchronic variants. The Gbaya form has typical variants in other dialects (e.g. *mbééréé*, *mbélé*, *mbé*, etc.), whereas the Banda form has no such variants. Besides, one is used predominantly by Gbayas and the other by Bandas.

6, sesame seeds: *núnú* [Banda] (2, 4, 11, 14, 22), *sùnù* [Gbaya] (18, 21), *sindl* [Yakoma?] (1, 3, 5-10, 12, 15-17, 19, 20, 23-27), *sóngbá* (13). Tisserant traces *sóngbá* to Nzakara by way of Banda, but informant 12 said that it was from Ngbandi. The words *sùnù* and *sindl* are very likely cognates; it is quite easy to explain the relationship between *u* and *i*, and between *n* and *nd*.

7, peanuts still in the skins: *bàrànzà* (6, 11, 12), *kwákúrá* (14), *yàwùndù* (22), *káráko* [Banda] (by all others). *Bàrànzà* is generally believed to be a Banda word, but Tisserant's only identification of *bàrànzà* is *Hygrophyla longifolia* Kurcz., which could hardly be mistaken for peanuts. Tisserant also gives the vernacular Sango as the origin of *káráko*, but Lekens does not have this word in his dictionary. *Kwákúrá* is probably related to the Banda word *akora* recorded by Tisserant for peanuts. The single informant who used *yàwùndù* was using the name of a specific type of peanut which goes by the name of the Cameroun city Yaounde.

8, grain of an uncultivated tree (*Amblygonocarpus Schweinfurthii* Harms. The pods are collected from the ground beneath the trees and broken up for the grains. These are used in the preparation of a sauce): *dàdàwá* (5, 19, 20, 27), *dàùdá* (9), *gónò* [Gbaya] (21), Sango unknown (3, 4, 18, 22, 25, 26), *kákéré* [Banda] (by all others). The words *dàdàwá* and *dàùdá* can not be traced with certainty; informant 5 claimed that *dàdàwá* was Boronu, and 13 that *dàùdá* was Arabic.

9, oily seed (either *Lagenaria vulgaris* L. or *Cucumeropsis Manii* Nand.): *kàndà* (14), *kpl* (15, see comments for 5), *mbéréké* (3, 19, 27, see comments for 5), Sango unknown (25, 26), *kóssò* (by all others). *Kàndà* is not to be considered a significant variation, because of the undependability of the informant. The only *kàndà* identified by Tisserant (for Banda/Togbo, Langwasi) is used of corn. *Kóssò* is the most widely used term, apparently coming from Banda. It is this word which is identified with *Lagenaria*. The *Cucumeropsis* identification is based on the use by two informants (26, 27) of *ngùmbé* in their vernaculars.

10, edible wild fruit (*Vitex* sp.): *biri* [Gbaya] (5, 18, 27), *árlá* [Banda] (8, 13, 14), *gbìgbì* (?) (17), Sango unknown (1-4, 6, 7, 9-12, 15, 16, 19-26). Informant 21 did not name the item but identified it by calling it *lé tí kéké tí ngòndà* "fruit of a wild tree".

21, sun-dried okra pods (*Hibiscus esculentus* L.): *dàrábà* (?) (15), *dòngó* (21, probably used in the generic sense, see 4), *vèkè* [Banda] (by all others).

22, edible leaf (botanical identification is uncertain. If this is *mbùnù*, it may be *Ceratotheca sesamoides* Endl.; but if it is *gósá*, then it may be *Corchorus* gen., cf. 31): *gósá* [Banda/Langwasi] (1, 2, 19, 24), *mbùnù* [Banda] (6, 8, 13, 14), *búrúgùtú* [Gbaya?] (27), *ngbúrú* [Gbaya] (22), *vèkè* (26, cf. 21), *bòlò* [Gbaya?] (23), *dàndó* [Banda?] (14), *àrigódò* [French *haricot*] (5), *dòngó* (7, 19, 20), *dòngó dòngó* (21), *dòngó yòngórò* (11, 12, 16), *dòngórò yòngórò* (15), Sango unknown (3, 4, 9, 18, 25). The extreme variation is

probably indicative of a difficulty in the identification of the leaf. It is nonetheless strange that the common semantic feature in most of these words is the mucilaginous characteristic.

23, small red peppers (*Capsicum annum* L. or *Capsicum fruticosum* L.): *mànzékè* [?] (3, 4, 14), *ndóngó* [?] (by all others). Tisserant is probably wrong in attributing *mànzékè* to Banda/Linda, since the first syllable *mà-* (as in other Sango words) can be attributed to some Bantu language, perhaps Lingala. Tisserant also attributes *ndóngó* to Ngbandi, but Lekens does not have this entry in his dictionary. In any case, the many different vernacular forms show enough similarity among themselves and with *ndóngó* that one is led to believe that the relationships are old. In other words, it may be impossible to distinguish a truly Sango word from a very similar vernacular word.

24, cola nut: *górò* [?] (by all informants). The universal recognition of the cola nut and the unanimity of the appellation is surprising indeed, when one considers the fact that cola-chewing is not at all wide-spread among the indigenous population. This habit is found primarily among the West African traders, but also to some extent among the male inhabitants of the larger towns, especially Bangui.

27, wild pepper (*Xylopia aethiopica* A. Rich., sold in the markets to cola-chewers but widely known for its use in the preparation of food): *màzindì* [?] (12, 13, 17), *simbá* [?] (23), *nzángé* [Isungu?] (27), Sango unknown (18-22, 24-26), not tested (1-11, 14-16). Tisserant attributes *màzindì* to Banda, but this is doubtful for the same reasons given for *mànzékè* above. Informant 20 simply described this fruit as *ndóngó tí ngòndà*, literally "pepper of bush", i.e. "wild pepper".

28, edible leaf (*Solanum aethiopicum* L.): *pásá* [Gbanu?] (22), *njènjó* [Gbaya] (19), Sango unknown (23), not tested (1-11, 14, 19-22, 27), *ngágo* [?] (by all others). Tisserant attributes *ngágo* to Banda, Gbaya, and Manza, but this is unlikely since other Gbaya words are known. It is probable that his informants gave him either the Banda word, in the case of Banda informants, or the Sango word, believing it was their own.

31, edible leaf (*Corchorus* gen., based on the informants' identification as *gósá* and *gbòlò*): *gósá* [Banda] (1, 2, 7-10), *gbòlò* [Gbaya] (5), *vèné* [?] (6), Sango unknown (3, 4), not tested (11-27). My notes indicate that, according to a Central African, *vèné* came from the Yakoma language; it is not found in Lekens' dictionary, however.

32, edible leaf (*Hibiscus sabdariffa* L., based on the informants' identification as *kókpa* and *zimà*): *kàràkànzi* [?] (5-7, 9), *kókpa* [Banda] (8, 10), Sango unknown (1-4), not tested (11-27).

33, edible leaf (*Cucurbita Pepo* L.): *kàwàyà* [Banda] (2-4, 7-10), Sango unknown (1, 6), not tested (11-27).

35, edible leaf (*Amarantus caudatus* L., based on informants' identification as *lumbú* and *mbúdé*): *gbùdù* [?] (1, 2, 4, 5), *lumbú* [Banda] (9, 10), *ngágóá* [Banda] (7), Sango unknown (3, 6, 8), not tested (11-27). The *ngágóá* identification is probably an incorrect one, for it is a dialect variant of *ngágo*, cf. 28.

## DISCUSSION

RICHARDSON: I would like to say that, although the parallel drawn by Dr. Samarin between Sango and Town Bemba in no way disagrees with the sense of my articles (1961, 1963, 1964) on the linguistic effects of urbanization, the situation is of course rather different in the two areas. Town Bemba, as its name implies, flourishes in an entirely urban context. It is the mark of the urbanite, of the élite, of the people who have 'been places' and can go back home and tell the villagers what it is to live the life of the bright lights.

Moreover, there are quite definitely some political overtones in this language. These emerge partly from factors attaching to a particular situation. The Bemba tribe lives in Northern Rhodesia. Bemba is a Bantu language and one of the six major vernaculars taught there in the schools. There are of course many other 'minor' languages. The Bemba have exhibited a remarkable degree of cohesion and also a spirit of opposition to colonialism. This is true of other tribes — but the Bemba have been very articulate about it. They have bred many politicians, trades-union leaders and businessmen. These form, in a sense, the spearhead of the attack. Many of the negotiations on political and trades-union matters are conducted in Bemba. I would not claim, however, that the language used in these instances is TOWN Bemba. Indeed its affiliations are rather with 'classical' or VILLAGE Bemba. Nevertheless, it is because of the feeling that Bemba is the language of those who seek to overthrow the old régime that a deviant version has been adopted in towns.

There is also a demographic factor favoring the adoption of Bemba in the Copperbelt. Most of the migrant labourers are of Bemba origin. However, even given the political and demographic reasons for the rise of a deviant version of Bemba as an urban lingua franca, it is still hard to see why one of the most complicated languages in Bantu Africa should have been chosen for this role. Apart from exhibiting an 18-class grammatical structure — which is typical of many languages of this region — it also has more than 30 one-word affirmative tenses and over 20 corresponding negative forms. In addition to this there are several compound forms which occur through a large part of the tense range. The usage of the correct form is determined, not only by time- and aspect-reference, but also by various syntactic criteria. Often the distinctiveness of several forms depends entirely on vowel length or tonal phenomena.

It is only when one considers the alternatives to Bemba that it becomes obvious that social rather than linguistic motives dictated the choice. Other tribal languages failed to appeal to the town-dweller because they had little or no prestige in an urban context. The only serious challenger was Nyanja — but this bore the taint of having been set up as the medium of communication in the Army and in the police force — the 'institutions of oppression'. Consequently, despite the fact that it is far more amenable to the foreign learner than is Bemba, its influence is rapidly declining. Cikabanga, a derivation of Kitchen Kaffir from South Africa, is a kind of pidginized

language with a vocabulary largely composed of Southern Bantu, Afrikaans and English words. It is no one's mother-tongue but is used as a 'work language'. Many people of European origin in Southern Africa consider it to be the ONLY suitable language in which to address a Bantu-speaking African. Thus, Cikabanga has acquired the reputation of being a 'master-to-slave' jargon which is totally unacceptable to the emancipated townsman. The only other possible choice was English. English enjoys the greatest prestige of all the languages in the Copperbelt, but owing to inadequate educational facilities, there are comparatively few who speak it well, and so a variant of Bemba was chosen as the most practical and prestige-conferring solution.

At this point I feel that I should indicate that while a knowledge of either English or Town Bemba confers great prestige on the speaker, the difference in prestige afforded by these two languages is one of character rather than of degree. In other words, a fluent speaker of English creates for himself an image of being not only a scholar but also one of the intelligentsia to whom eventually no door will remain closed, whereas the ability to speak Town Bemba shows to one's fellows that the speaker has made the necessary adjustment between the old and the new with equanimity and even with a certain *panache*.

It has already been pointed out that both Sango and Town Bemba lack homogeneity. It is probably truer to say that Town Bemba is an all-embracing term for the general corpus of speech produced by Africans of varying linguistic and social backgrounds in their attempt to reach the 'ideal' standard of Town Bemba. From statements gathered in the field, it appears that this 'ideal' really exists in the minds of town-dwellers and could briefly be defined as 'the type of Bemba spoken in towns by those who have Bemba as their mother-tongue'. This is not to say that this 'ideal' is identical with Village Bemba. Indeed, even the EXPERT speakers of Town Bemba deviate to a limited degree from village usage, as for example in the re-assignment of certain words to different grammatical classes. The chief differences, however, lie in the lexicon and in the style. Town Bemba prefers the imported word to the local product. Furthermore, when traditional words are used it is often with so violent a semantic 'twist' that traditionalists fail to understand them. The style of speech must be racy, allusive, and amusing. New terms are born and die with bewildering rapidity, but the really proficient speaker must keep abreast of the times, thereby proving to others that he is an initiate of the inner urban circle and that THEY are still emerging from the dark ages.

In conclusion, it should be noted that many of the features I have described are not specifically African but can probably be paralleled in any society where certain elements feel the need of a linguistic 'uniform' to rally them against the others.

P. Ivić: It seems to me that the nature of the instability and diversification in Sango and in Town Bemba is not the same. In Sango we have to do with a kind of hyper-correction. Speakers of dialect A think that elements of Sango, which are identical with their own language, really do not belong to Sango. Therefore, they use the

form of dialect B, and vice versa. In Town Bemba we have to do with a breakdown of the grammatical pattern. The contact and interference between a number of dialects with very complex and very different structures leads inevitably to an abolition of many existing distinctions — of course not in the same way throughout the whole area.

GARVIN: Thanks to the presentation by Dr. Samarin, I have an idea for summing up what one deals with in sociolinguistics. There are two clearly defined language situations that can be treated only in terms of this hyphenated discipline. One is the standard language problem; this is linked, to some extent, to the question of language and dialects as defined in a social sense, rather than by the Bloomfieldian criterion of intelligibility. The second is the question of lingua franca, which was raised in the Sango situation. My own work has been in the area of standard language more than that of lingua franca, so I am tempted to use some of the standard language criteria, apply them to the Sango situation, and point out the differences. One salient point is that there seems to be no codification in this Sango language, which means that its speakers form their notions of the language in terms of their own folk-linguistics. They apparently have a sort of folklore about what Sango ought to be like, and there is no code for them to use as a frame of reference. It seems that the overriding function here is the prestige function, while the others, the separatist and unifying functions, are less significant. And the prestige function is apparently assigned to something that is not the vernacular. Consequently the folk-linguistic conception of the speakers apparently is defined entirely by contrast, which gives you *des entités négatives et oppositives* in the Saussurean sense, in a somewhat extreme form. This seems to me not to be the case of the usual pidgin, which is not interpreted negatively, but in some sense positively.

HAUGEN: I had the opportunity of reading part of a thesis which has been worked out under Dr. Samarin, and I noticed one point which seems to me to have some interest. I think there is a good deal to be said for the point of view that this is not a language. According to the thesis, the whole language, as analyzed from over 37,000 words of running text, has only 482 words.

SAMARIN: Less, actually, because we put words into the index that we, as speakers of the language, had discovered elsewhere. So there were less than 482 in the corpus.

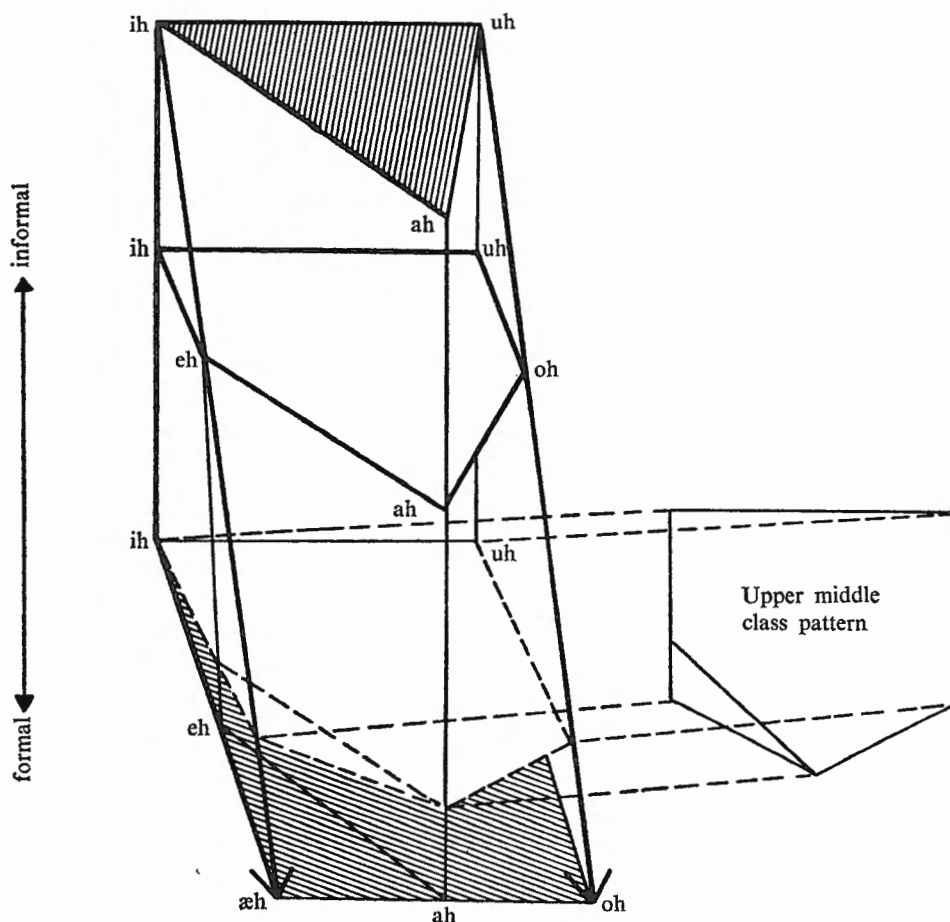
HAUGEN: This comes awfully close to that old fable about the language in Africa that had only 600 words. And this may be the place where that came from! The story has been rejected by competent linguists for so long that I hesitate to accept this as a language until this particular point is cleared up. The texts that you collected seem to be largely loan words strung together by a small number of function words. Now this can conceivably be a language, but some of the things that Paul Garvin mentioned about standardization are certainly applicable here. We may have a koiné or a pidgin in the making. However, your mode of deriving the data here may have influenced this particular problem to some extent. I would like to know how the vocabulary of this idiom compares with that of the vernaculars. Is it as much smaller as this seems to make it, on the assumption that the vernaculars have a normal size vocabulary (whatever that is)? As for the negative influence, I think is certainly

present in every standard language and koiné, because people learning a standard language are afraid to be using the words that are natural to them. So they avoid them and adopt entirely different words. In this way a kind of consensus is gradually arrived at, and I don't think the codification is necessary at all. A formal codification is an entirely secondary thing. Informal codification comes into being among the speakers as the result of a consensus and of some kind of prestige nucleus among the speakers, which apparently is not present here. French is the prestige language, and whether Sango is a prestige language is a matter of debate.

SAMARIN: Sango does have, as we see in the texts, very few words. And many of the French words could be eliminated without hampering the language at all. Some of these are just synonyms. For example, the French word *mais* is used in Sango, with no distinction from *na* "but". In fact, they say *mε na*. Of course, other words like *commander* are used, and there are no Sango equivalents. But Sango is not French interlarded with African words. Most of the French borrowings are nouns; there are very few borrowed function words, and in a single text, especially a conversation or a fable, there is a very low frequency of French words. In our 37,000 word corpus there are 508 French words. But all I can say is that it is a language, because I can spend hours with my friends talking about all kinds of things, describing Los Angeles and how many cars are on the streets, and so on. But we do have some difficulty talking about theology. The whole Bible has been translated, though sometimes with great difficulty, and of course it is not like an English translation (see fn. 3).

FERGUSON: The kind of negative selection found in Sango occurs very commonly, although in a minor way perhaps, in a diglossia situation. If the so-called 'high' variety in a diglossia situation has two alternative ways of saying something, and one of them happens to be like the local dialect, a speaker will normally reject the one that's like his local dialect when he writes or uses the higher form of the language. In the last 100 years, this has actually pushed some words almost completely out of Classical Arabic. For example, you used to be able to say *rāḥ* or *ḍahab* as the verb "to go" in Classical Arabic. But *rāḥ* is the normal word in many dialects, and people don't want to use that in written Arabic. So now written Arabic uses *ḍahab* almost exclusively as the verb "to go". This happens not only in lexical items but even in grammatical features. There are two ways of making the negative of the past in Classical Arabic. One of these ways is very much like the way that is used in many local dialects, so that way is used much less now than it was in older texts. This kind of negative selection, I think, occurs not only in Arabic but in languages like Swiss German, Haitian Creole, and so forth.

LABOV: It might be useful to compare a diagram of the New York City situation with the sociolinguistic situation that Dr. Samarin has just described. This is a three-dimensional sketch of the in-gliding vowel system in New York City. (See diagram.) At the top is the plane of casual speech, the vernacular as used by the younger generation of New Yorkers. There are three phonemes: /ih/, /uh/ and /ah/. This is the system which reveals the dynamics of linguistic interaction in phonological



space as described by Martinet and by Moulton. At the bottom is the plane of the most formal style, the type of response given when you ask for isolated words as in Style D of our interview and the elicitation method of Dr. Samarin. Here we have six phonemes: /ih/ in *here*, /eh/ in *there*, /æh/ in *bad*, /uh/ in *sure*, /oh/ in *sure*, /ah/ in *bar* and *guard*. This is hardly a coherent system: /æh/ and /oh/ fluctuate from short vowels to long, appearing and disappearing, and the system as a whole appears and disappears as the subject uses more or less *r*-pronunciation. It would be difficult to describe this plane as a linguistic system. The structural elements which are most consistent for the diagram as a whole are the upper plane of casual speech, and the direction of shift downward. This shift is governed by the prestige pattern, shown at the left. The hyper-correction, similar to that referred to by Professor Ivić, is shown by the fact that the end product of the stylistic shift goes beyond the prestige model. New Yorkers aim at this model but go past it. There is a striking parallel with

Dr. Samarin's model in that there is little agreement, little uniformity, and little coherence in the elements of the most formal system.

STOCKWELL: Could you clarify this further with examples?

LABOV: Well, the three elements on the left of the bottom plane represent the word classes of *here*, *there*, and *bad*. In the most casual speech of most younger New Yorkers, the same phoneme is used in all three. So you have the same vowel in "I shaved my [bɪ:əd], I cut my [hɪ:ə], I had a [bɪ:əd] cut", or "I shaved my [bɪ:ɾd], I cut my [hɪ:ɾ], I had a [bɪ:ɾd] cut." It's hard for you to hear them as the same, because your morphological interpretation gets in the way. To give one actual example, one informant talked about giving her little girl a [hɪ<sup>ə</sup>-cut. No one misunderstands her, or even notices that she is saying [hɪ<sup>ə</sup>] when you play the whole tape, but if you cut off the word *cut*, the listener hears the first word as *here*. In other words, here is only one front vowel, one low vowel, one back vowel, and an occasional center vowel in *her* and *were*. This describes the speech of large numbers of younger New Yorkers, under 40 years old, in casual style where pre-consonantal and final [r] does not occur.

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## STRUCTURAL IMPLICATIONS OF RUSSIAN PRONOMINAL USAGE

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Studies in the relation between language and culture have sometimes focused on how a way of life is categorized through words. The results are often of enormous value, as in the case of a sensitive dictionary. But such lexicographical inventories typically lack a basis in theory.

A second kind of study may focus on the connections between obligatory grammatical categories in the linguistic system, and axiomatic distinctions in the abstract thought of a people. Controlled demonstrations are sometimes possible, as for Classical Greek, and the conclusions are often intellectually challenging, as in Whorf's arguments about Hopi (1956). But one often finds, on closer inspection, that the causal relationships are little more than suggested or asserted (Jakobson 1959: 142-3).

In short, language and culture studies tend to oscillate between comparatively descriptive correlations of "words and things", and comparatively tenuous correlations between grammar and metaphysics.

Perhaps more interesting than the lexicographic extreme and more fruitful than the philosophical one would be complex descriptive theories based on explicit premises. This article is an attempt to construct one such theory. In the first place, I assume that one should distinguish explicitly between four main categories of phenomena, which can be labelled society, culture, speech, and language. "Society" is taken to refer to the regularities that may be observed or measured in the relations between the individuals and groups of some bounded and interacting population. "Culture" is used to denote the structured set of historically derived explicit and implicit norms, values and feelings that are shared and transmitted by the members of a society. "Language" refers to the set of obligatory grammatical and semantic features, the relative distribution of invariant units, and other dimensions of an inferred structure. "Speech", finally, refers to facts of frequency, examples of usage, and other audible, measurable, concrete phenomena. Both speech and society in the slightly technical sense here used are behavioral: they can be more or less directly described. By the same token, language and culture are structural, consisting of oppositional, contrastive relationships between the units of a system.

In the second place, I assume that all four of the behavioral and structural phenomena covary with each other. My goal will be to show how one phase of speech usage is determined by principles of what might be called "social culture" — that part of

the cultural system which influences or governs individual and group relations. In addition, contingent relationships will be pointed out between abstract linguistic patterning and the abstract patterns of Russian social culture.<sup>1</sup>

In this paper I shall first briefly summarize the relevant linguistic theory, pointing out those linguistic attributes of pronouns which co-vary with socio-cultural and psychological phenomena. There comes next a brief sketch of the groups and conceptual categories in Russian society and culture, with a focus on the three social classes for which the textual evidence is particularly good: the gentry, the peasantry and, as a small middle class, the *meshchánstvo* and *raznochíntsy*. The social culture and pronominal structure are then related to three overriding characterizations of nineteenth century Russia. In the succeeding part I outline how selection of the appropriate pronouns co-varied with the genealogical distance, emotional 'solidarity' and eight other discriminations. I then discuss in considerable detail the patterns governing the three types of symmetrical and asymmetrical usage of the second person singular pronoun. This leads directly to eight case studies of pronominal 'switching' and what is here called 'breakthrough', notably in *Crime and Punishment*. A final set of examples illustrates what will be called the expressive and the latent *ty*. En route through these cases, the relative power of the underlying discriminations is repeatedly examined. The last part of the article deals with certain advantages and limitations of this kind of semantic analysis.

#### BACKGROUND AND EVIDENCE

Study of the Russian system can benefit from the numerous and original analyses of both the linguistic and sociological aspects of pronouns. Linguists such as Jespersen (1924) and Bloomfield have recognized and discussed the peculiar combination of attributes shared by pronouns, and in more recent years a number of original papers have presented elegant networks of underlying dimensions (Conklin 1960: 134-35). Social anthropologists and social psychologists, on the other hand, have shown how pronominal usage is connected with attitudes and behavior (Brown and Gilman 1960), and philologists have produced accurate and carefully documented histories of the usage in German, French and other languages (e.g. Fay 1920). My present intention is to contribute to this small but steady flow of scholarship by demonstrating how the purely linguistic properties of pronouns were realized in nineteenth century Russian, and, secondly, how these properties and the pronominal usage did in fact co-vary with psychological, social and cultural phenomena.

The evidence for social behavior, culture and pronominal usage happens to be

<sup>1</sup> Sociolinguists and linguists often do not differentiate between society and culture in the above sense. In what follows "societal" refers to society whereas "social" refers to "social culture", but is sometimes used generically.

excellent for Russia. A valid understanding of the society and culture can be extracted from three brilliant *magna opera* of two to three volumes each, composed by three very different men: a German baron and economist (von Haxthausen), a French sociologist and man of letters (Leroy-Beaulieu), and a peripatetic Scotch journalist (Wallace); other articles and books written over the past century round out the picture.

A full and balanced interpretation of the pronominal usage may be gotten from the creative literature; the novels, in particular, adequately define most protagonists and the contexts of most speech events. For example, Tolstoy gives the usage of the high aristocracy, which Dostoyevsky complements with the patterns of transitional, urban classes. Gorki presents a panorama of proletarian and artisan customs, Leskov covers the clergy, and Zlatovratsky and Sholokhov provide truly ethnographic detail on the peasants and Cossacks, respectively. Some authors, notably Dostoyevsky, also give a vast panorama of contacts between persons of divergent background and rapidly fluctuating emotions. My sample of over eight thousand pages of text is replete with evidence, because the pronoun must be at least implied every time a verb refers to an addressee. Russian literature was written and read during the last century on the assumption of a social structure which no longer exists but which, as a historical relativist, I feel strongly motivated to reconstruct. Needless to say, pronominal usage is one of the serious losses in any English translation.

The evidence on pronouns is felicitous on quite specific, aesthetic grounds. The Russian novel was always 'realistic' in its concern with burning moral and political issues, with differences of social or bureaucratic status, and with the nature of human thought and emotion.<sup>2</sup> The literature involved the culture of the readers themselves, or of the groups they understood through personal experience, such as the peasants, or of others such as the gypsies with whom they had had contact; ethnographic accuracy was an aesthetic imperative. For instance, Tolstoy was abnormally aware of the pronominal symbolism of social differences, and was prone to interject passing comments on the usage of protagonists in his own novels. It is true that social statuses and strata are unrealistically portrayed in some creative literature, and that plots and protagonists may be manipulated in 'unreal' ways in conformity with literary conventions or the creative imagination of the writer, but it is equally true that pronouns, since they belong to what linguists call 'background phenomena', are not misused in artificial ways any more than are grammatical paradigms.

On the contrary, a *sine qua non* for a writer is that his usage of language,

<sup>2</sup> As Gerald Kelley has pointed out in a personal communication, this contrasts with the United States, where much of the literature functions to inform the reader, to create expectations of usage and behavior in subcultures not known through personal experience. The critical notion of "realism" is hardly adequate as a cover term for the Russian novel. In the present context, suffice it to say that the literature did provide a wealth of insight into character and culture.

no matter how original in syntactic style or choice of word, be generally acceptable as regards the obligatory, covert categories of cultural and linguistic structure. Writers conform to the norms of their culture in the sense that their work must be readily intelligible in terms of those norms. The uniquely original figures in Gogol and Chekhov do not jar the average reader in their use of pronouns *qua* pronouns. In other words, an original character can only be such if one knows the norms by which he is judged.

#### PRONOUNS IN LINGUISTIC THEORY

Every language includes a small but logically differentiated class of words such as *you*. Whereas most words can be defined by other words of the same kind, the small class of words such as *you*, or the article *the*, or the number *two*, can only be defined through the entire class that — in the case of the three examples — can be addressed, named or enumerated; in short, the meaning of these words is the generic meaning of large grammatical classes for any one member of which they can be substituted. These so-called “substitutes” are more abstract and inclusive in meaning than any of the words to which they may refer in some particular context; for example, *you* is more abstract and has greater freedom of distribution than *brother*, although both may be variously employed when addressing the same individual. The substitutes are usually more frequent and appear in a greater number of constructions than any of the individual words they may replace.<sup>3</sup>

By the same reasoning, a substitute depends more for its meaning upon the environment or context of the speech event than do other parts of speech; devoid of context, the person or thing referred to by *you* is unknowable. In the terminology of the Prague School, the substitutes are “less marked” than most parts of speech because they convey less specific information than, for instance, nouns and adverbs.

The meaning of a substitute is likewise simpler and more constant than that of any of the thousands of words it can replace: “In their class-meaning, substitutes are one step farther removed than ordinary forms from practical reality, since they designate not real objects but grammatical form-classes; substitutes are, so to speak, linguistic forms of the second degree. In their substitution-type, on the other hand, substitutes are more primitive than ordinary linguistic forms, for they designate simply features of the immediate situation in which the speech is being uttered” (Bloomfield 1933: 250). The quasi-linguistic systems we call logic and mathematics are to a significant degree formalizations of the relationships of number, space, level, attribution, and so forth, that are implied by the class of substitutes. To conclude, the substitutes are termini for several grammatical dimensions, and they imply deep levels of gram-

<sup>3</sup> I am using “word” rather than “morpheme”, both to increase intelligibility and because I do not think that the distinction is important in this context.

matial information. They belong to what Hockett (1958: 246-67) has called the "grammatical core".<sup>4</sup>

Among its substitutes every language includes a set of pronouns, a paradigm that requires inclusion in even the briefest grammatical sketch. Partly because of their resistance to borrowing and change, the pronouns also figure in the so-called 'non-cultural' or 'basic' vocabulary (Swadesh 1951). But whereas most such 'non-cultural' words refer to universal experiences — *moon, finger, sleeping* — the pronouns refer to universal properties of speech itself or, to speak more precisely, to properties of the process of communicating with symbols, since there must be some speaker (e.g. *I*), some addressee such as *you*, and some topic of discourse. Hypothetically, of course, nouns or names could designate these invariant components of the speech event, and that is precisely what two-year-olds do. But this is repetitious and inefficient; the aforementioned simplicity and generality of pronouns is related to their presence without exception in the economy of all languages. For similar reasons, the use and switching of pronouns, as will be elucidated below, can reflect almost kaleidoscopic changes in the implicitly universal relations between two protagonists in a work of literature.

Consonant with their exceptional status in language, the pronouns also tend to display grammatical and even phonological peculiarities. For example, the English phoneme /ð/ only appears initially in the demonstrative pronouns such as *this* and in a few other words, most of them substitutes. The Russian phoneme /e/ appears initially in only one native word: *éto* "this". Unfortunately, the textual nature of the evidence used in this paper precludes adequate treatment of the many habits and patterns by which pronouns are phonetically modified through subphonemic variation and distinctive modulation of the voice.

Pronouns are also linked in quite special ways to the systems of gesture and intonation currently called 'paralinguistic'; the act of pointing, for instance, can be substituted for anything that can be pointed at, and can even substitute for a pronominal substitute, as when the levelled index finger and silence replace *You!* Pronouns, like intonation and body movements, can act as interjections, or exclamations, or other kinds of highly expressive or evocative signalling (Bloomfield 1933: 250). This is notably the case where the language, such as Russian or Spanish, uses conjugated verbs to convey the number and person, and where the subject pronoun may signal emphasis, personal involvement or at least partly redundant information. For example, in Russian *ty otpustí menjá* "Let me go!", the *ty* pronoun is implied by the verb; therefore, the addition of an overt *ty* merely brings out what the speaker or narrator thinks or feels about the addressee.<sup>5</sup> Finally, the singularly close relation

<sup>4</sup> These chapters in Hockett show, if little else, that much work remains to be done on the specification of "grammatical depth" and "core". But I think the notion is essentially sound and fits the discussion here.

<sup>5</sup> The emotional signal is different in English parallels such as "Let me go!" versus "You let me go!"

between pronouns and paralinguistic systems suggests that both imply deeper levels of emotion than most other linguistic phenomena.

Every language includes among its pronouns a smaller set of personal pronouns that invariably imply at least the two minimal dimensions of person and number; the singular-plural contrast reflects a fundamental dichotomy in human culture, and the notion of person rests on the fundamental, three-fold nature of speech discussed above. The minimal and relatively 'objective' meanings of number and person can have culturally specific nuances and connotations, of course. Several other dimensions, in addition, are either limited in distribution, such as the inclusive and exclusive plural, or imply culturally arbitrary categories such as gender. The selection of pronouns may also be determined by a third type of obviously cultural discrimination, such as class or caste membership, or the formality of a given situation. The second person pronouns, used to the addressee, are particularly delicate indicators of the socio-cultural and personal relationship between two speakers. Study of the factors determining Russian *ty* and *vy* turns into the inference of an intrinsic view of how the individual expresses himself within and through a given status system, and leads one to an ego-oriented perspective of the social culture that complements conclusions about group behavior and group norms.

Sets of second person pronouns are of theoretical interest because they link the abstract properties of a basic grammatical paradigm in a given language to a second matrix of culturally specific components that are both very frequent, and of emotional and social importance. In other words, pronominal usage affords us an analytically accessible link between the domain of the obligatory categories of grammatical structure, and a second domain of semantic categories which reflect the similarly obligatory lines in the social culture; the second person pronouns occupy the boundary between 'deep grammar' and the very innards of the society. For this reason, their study may emerge as a comparatively fruitful ground — a felicitous case — for explorations of one component of the so-called Whorfian hypothesis.<sup>6</sup>

At least in theory, the second person pronouns cannot be treated in isolation because they function in interrelation with other sets, notably kinship terms, proper names, and official ranks. In Russian, numerous kinship terms of address and many other quasi-kinship terms such as *kum* "ritual co-parent" were frequent in conversation, particularly among the peasants. All classes of Russians interjected kinship terms such as "brother" or "little mother" (*mátushka*) when consciously or subconsciously trying to create an informal, congenial atmosphere with non-relatives. And there were numerous combinations of proper names, ranging from nicknames, to diminutives, to the first name alone, to the first name plus a fixed epithet ("Mikhail the Wolf"), to the name plus an informal or formal patronymic (Ivanych as against

<sup>6</sup> Norman McQuown has been emphasizing the sociolinguistic significance of substitutes for several years. I have profited from several discussions with him on this subject. Otherwise, I retain hope but extreme skepticism about demonstrating any pervasive causal relation between social structure and grammatical patterns (e.g. Russian aspect), à la Whorf.

Ivanovich), and so on up the line. In addition to words for age, occupation, and so forth, there came a vast panoply of official designations. These means of address had to be calibrated with each other. For example: "Aren't *ty* joking, Foma?" — "In the first place, I am not *ty* ... but *vy*, and don't forget it; and not Foma, but Foma Fomich" (Dostoyevsky, *Village of Stepanchikovo*, 472).<sup>7</sup> All the terms of address also had to be calibrated with the partly independent and equally copious terms of reference; the total number of combinations was astronomical. Gogol, referring to all the possibilities, was thus literally correct when he wrote facetiously in *Dead Souls* (35-6):

It should be said that if we in Russia have not yet caught up with the foreigners in some things, we have long over-taken them in the means of address. It is impossible to count all the shades and niceties of our means of address. The Frenchman or the German will never grasp or understand all the particularities and differences; with almost the same voice and language he will start to speak with a millionaire, and with a petty tobacco vender although, of course, in his soul he is appropriately base to the first. It is not like that with us. Among us there are wise fellows who will speak altogether differently with a landlord having two hundred serfs than with one who has three hundred, and with one who has three hundred they will not speak as they would with one having five hundred, and with one having five hundred, again, not as with one having eight hundred; in a word, although you go to a million, they will always find shades (of difference).

#### SOCIAL CATEGORIES AND SOCIETAL GROUPS IN RUSSIAN CULTURE

During the nineteenth century the levels and compartments of Russian society were more richly chambered and ostentatiously marked than in most of Western Europe. Social differences were signalled by manner, dialect and hereditary occupation, and upheld by a legal and governmental system of prescribed qualifications, rights and obligations. In fact, the differentiation was so multiple that the resulting social groups and cultural categories cannot be ranked in terms of any two dimensions; for example, village priests shared with the gentry the exemption from corporal punishment, but in a general social and political sense they were inferior to wealthy peasants and Cossacks.

Within or beneath the cultural system, labelled by language and buttressed by law and tradition, there were of course many statistical differences and variations in actual behavior. Peasants and workers, for example, made up over ninety percent of the population, and the overwhelming bulk of Russian was spoken by them; in contrast, the priests were a tiny minority, and the fully qualified, blue-blooded aristocrats belonged to less than one hundred large families. In addition, the cultural strata were constantly being broken by mobile individuals, as when a wealthy free

<sup>7</sup> Page numbers in references to Russian fiction are from recent editions that I have consulted; see the listings of these works at the end of this paper.



peasant gained admission to a guild of merchants, or the daughter of a wealthy merchant married an indebted young hussar with an illustrious pedigree. Other groups were geographically peripheral or socially intermediary, as in the case of the "townspeople" (*meshchánstvo*) discussed below. But despite societal differences and individual behavior, the vast majority of Russians at any one time could be assigned to one of a small number of groups set off from each other by legal status, more or less hereditary occupation, distinctive ritual, and dialect.

Let us review some of the criteria differentiating major categories. Economic criteria reserved to the gentry the exclusive right of owning agricultural lands (until 1861), and limited the capital holdings of petty artisans and merchants. Written laws and statutes created other, quite independent boundaries; until 1861, only wealthy merchants, priests and the gentry were exempt from the poll tax on males, from the common draft (which gradually decreased from 25 to four years), and from the horrors of corporal punishment with the knout. In accord with another type of legal demarcation, each social group had its own courts for the adjudication of certain internal affairs, notably in the case of artisan cooperatives (*tsekh*) and the comprehensive customary law of the peasants. Occupation was generally hereditary during the first half of the century and served to segregate the population into four major groups: the peasants, artisans and proletarians, who lived mainly by manual labor; the priests, who supervised the Orthodox ritual, especially individual rites of passage; the merchants and businessmen, managing the flow of capital goods and credit; and, finally, the gentry, who upheld or sought to create familial traditions of education and upper-class mores by the earnings from their landed estates, and by continuing service to the nation as officers or bureaucrats. Until 1861, the legal and occupational structure was further subdivided by the institution of serfdom, setting off most of the peasants and some of the artisans and workers as categories of things which could be bought and sold, or married, or often mistreated at the whim of the patriarchal proprietor. Political power and direct access to the sources of authority in government were largely limited to the gentry, wealthy merchants, and *raznochintsy*, and typically denied to the great masses of peasants. Finally, the many sections of this quasi-Oriental, quasi-feudal, superficially modernized culture were marked off by the most intimate symbols of custom and dialect, notably in the case of the clergy, whose speech was saturated with archaic Old Russian and Slavonic elements. In sum, various norms and patterns of the culture fractured the society into discrete groups. Verbal exchange between them gave life much of its salt and stimulation.

The following sketch of social divisions is mainly concerned with the Great Russians (e.g., not with Ukrainians), and with the four major groups for which pronominal usage is well attested in the sample used: the peasants, the gentry, and two intermediate classes, the *meshchánstvo* and the *raznochínstvo*.

During the first part of the century most of central and northern European Russia was already inhabited by peasants, either privately owned, or state serfs (about one quarter), or freemen, located for the most part in the northeast. Congruent and rather

sharp lines of dialect and custom did distinguish the Great Russian peasants of the north from those of the central zones. But because of the recency of their expansion, and their geographical mobility, they had not become as differentiated internally as the German or Italian peasants. Russian peasant households ranged in size from single, immediate families to huge corporations of thirty or more individuals. But the average was between seven and fifteen, and the anticipated norm was a patrilocal aggregate that included some daughters-in-law and at least one adult man in residence. Such households were coöperatives, the women rotating domestic chores while the men tilled the soil together or labored in "home industries" or distant *tsekhi*. Three to fifteen or more households were aggregated into communalistic villages (*mir* or *obshchestvo*), with democratic governments that reallocated the land equitably every three to twenty years. The smaller communes were essentially extended kinship groups of peasants who were either linked biologically or at least thought of each other as "relatives". In short, the commune was a corporate group with diagnostic rights and an internal organization and degree of human solidarity not found in other strata.

The peasants shared a distinctive ethos. Because the soil was poor, and because redemption payments for the land after 1861 were often excessive, most peasant men spent months and even large portions of their lives earning extra rubles in distant factories, mines, and the transport industries. In the cities such teams of married men lived in workers' coöperatives — the *tsekh* or *artél'* — which retained close ties with their communes; but such workers were peasants in outlook, notably in their sense of fraternal and communal solidarity, and were generally no further removed than "third cousin or third uncle". Finally, every large household periodically had to send forth a young man for military service. To such occupational and geographical mobility were joined the constant splintering of villages and their tendency to expand eastward into virginal lands. A gregarious, fraternal spirit joined the peasants to each other. They, like the Cossacks and proletariat, lived in what might be called a familial universe where almost everyone was addressed with the informal pronoun, *ty*.

At the opposite reach of the social scale stood the gentry, or *dvorjánstvo*, a mere one and one half percent of the population, bifurcated, in turn, into the hereditary kind, with about six hundred thousand members (Leroy-Beaulieu 1889: 319), and about three hundred and fifty thousand of the "personal kind", which came automatically with service even in any of the lower ranks of the administration. Within the *dvorjánstvo* about one hundred families formed a sort of elite that partly resembled the true aristocracies of western Europe; some had genealogies going back to Viking conquerors of the ninth century, but others were only a few generations away from the successful general or bureaucrat who had founded the line. Most of the top posts in the army, the diplomatic corps, and so forth, were filled by persons bearing one of the distinguished names. They controlled most of the large estates, often running to thousands of acres, and, until the Emancipation, to hundreds or thousands of human chattel.

On the one hand, because of their informal ties with the masses, their autocratic rights over the serfs, and other less obvious reasons, the high aristocrats were not infrequently prone to extravagant barbarisms. On the other hand, they enjoyed the cultivated manners that stemmed from excellent lycées, foreign travel, service in the honor regiments, and a life divided between gracious country mansions and the high society of the capitals, so luminously depicted in *War and Peace*. Lacking formal and legal boundaries, they tended to set themselves off by often subtle combinations of birth, wealth, manners, intelligence, official rank and, above all, a mastery of literary French, which was well spoken even by much of the provincial gentry; as Leroy-Beaulieu (1881: 361) so aptly says, "la noblesse russe se pique de civilization", and, at another point, "une aristocratie légalement ouverte à tous ne saurait s'entourer d'un rempart plus efficace. Le français était devenu une sorte de passeport mondain..."

French determined Russian pronominal usage. Only *ty* had been employed till about 1700, whereas "... the French manner of address to one person in the plural number appeared in the eighteenth century and rapidly became current among the educated circles" (Isachenko 1960: 414). By 1800, more or less when French was instituted as a "passport", the use of the plural *vy* as a formal and respectful singular, on analogy with French *vous*, had become established and launched on its independent course, although subsequently reinforced and subtly influenced by the pervasive French bilingualism and constant exposure to French novels and plays.<sup>8</sup> But let us return to our characterization of the gentry.

Despite the proverbial ostentation of spendthrifts abroad, and apart from the few millionaires who controlled thousands of serfs, most *dvorjáne* were not rich; by the Emancipation, 41,000 of the slightly over 100,000 landed proprietors owned less than twenty-one male serfs, and only 3,803 owned over 500 (Wallace 1905: 280). Two thirds of all agricultural lands was mortgaged or otherwise in lien to rich merchants, bankers, and the like. The equal division of land among the male heirs, and the need to provide large dowries, led to incessant economic change and to over-all instability. At any time some individuals were slipping downwards into the mixed classes, or ascending vertically into the elite, or shifting about horizontally through the many niches of the hierarchy; the *dvorjánstvo* was an open group, lacking both the genealogical rigidity and the inbred hauteur and exclusiveness of nobility in the west. In fact, most of the *dvorjánstvo* were either peripheral to the great families, or consisted of smaller, poorer and less prestigious or successful lineages, either living out their lives on small estates or striving and intriguing with comparative energy in the imperial service.

<sup>8</sup> My hypothesis is that such rapid diffusion of a foreign pattern, and rapid semantic shift, were possible because of the profound changes in certain basic culture patterns that had resulted from the Petrine Reforms. Myles Dillon and Martin Murphy have pointed out that, in the Scotch Gaelic of the New Hebrides, Keltic *śu* formerly denoted the second person plural, but was applied as a second singular formal pronoun, on the model of French *tu* and *vous*, during the period of intense French influence in the eighteenth century. The usage has since spread from the gentry, and *śu* generally is used as a deferential form. This case thus parallels the Russian one very closely.

*Dvorjǎnstvo* status always depended heavily on service. After the reforms under Peter the Great (beginning about 1700), and until the mid-eighteenth century, any family which failed to serve for two consecutive generations lost its status. After Catherine's reforms a trend set in for the gentry to bifurcate between those who actually possessed landed estates, as against those who depended primarily on civil or military service. The first were often more cultivated, but also conservative and fearful of social change; the second were comparatively authoritarian and legalistic, but more aware of modern social and political ideas. In any case, civil servants or "serving people", as they were called, always aspired to landed estates, and every generation or two a family of landlords would send at least one son into government service. "The bureaucrat was only a nobleman in uniform, (and) the nobleman was only a bureaucrat in a dressing gown." Both the landlord and the official depended ultimately on the state and the legal person of the Tsar as the ultimate source of both their rights and their land. By the same token, the Russian term *dvorjanín* is not accurately rendered by "gentryman, nobleman, aristocrat", or any other English, French or German gloss.

Clearly, most Russians were either peasants or *dvorjáne*, and most Russian was spoken by persons in these two categories. But brief discussion is also merited by two additional, intermediate classes.

First, the "mixed group", or *meshchánstvo* — the term is as distinctively Russian as "Cossack".<sup>9</sup> The usual *meshchanín* was an urbanized artisan or petty merchant or trader, not permitted, in any case, to possess capital assets over six thousand rubles and, until the Emancipation, subject to the draft, corporal punishment and the poll tax. The most diverse types were recruited into this class: emancipated house serfs, retired soldiers, ruined or disinherited gentryfolk, peasants ostracized from their communes, and multifarious traders of "German", Tartar and other origins. Though small in relation to the peasant masses, they made up the majority of most cities, and, in 1875, about eight percent of the total population. Their style of life ranged enormously, from fairly educated families on the verge of entering the gentry through marriage or service, to the great bulk who were basically urban peasants, dwelling in large, patriarchal households and observing the ancient traditions. On the lower periphery of the class hung many impoverished and demoralized creatures — petty criminals, alcoholics, and the like. The coarseness and violent conflicts and contradictions within many *meshchánstvo* families are inimitably portrayed in Gorki's *Childhood*.<sup>10</sup>

There was also a second intermediate and relatively amorphous class. During the early eighteenth century there developed a sort of 'social et cetera' of persons difficult to categorize for purposes of taxation as belonging to any of the major categories

<sup>9</sup> Although the notion of "Cossack" is distinctively Russian, the word derives etymologically from the Turkic dialects of Central Asia.

<sup>10</sup> *Meshchanín* derives not from *meshát* "mix", but from Polish *mieszczanin* "burgher", which derives in turn from *miasto* "town" (Vasmer 1955: II).

(Becker 1959). These legal and administrative left-overs were termed *raznochintsy* — persons of varied rank (*chin*). Under Catherine the same term was more widely applied for the rapidly rising “new proletariat of the bureaucracy”, which by 1811 included twenty-two percent of the population of St. Petersburg. During the first half of the nineteenth century the still more general reference was to any non-gentry with the equivalent of a secondary school education, which in practice characteristically included the sons of middle and low ranking bureaucrats, or younger sons of the gentry, merchants or clergy, and many ambitious scions of the peasantry. Such persons of diverse origin made their living as bureaucrats, school teachers, journalists, lawyers, doctors and in certain other trades and professional crafts. They were not subject to the poll tax or corporal punishment, they lacked *dvorjanstvo* status and could not own farm land, and they were not enrolled in guilds, corporations and other groups as were the merchants and peasants. From an entirely realistic if negative definition, the *raznochinstvo* consisted of persons not assignable to one of the more definite categories; just as it was comparatively unmarked by customs, dialect, ritual or law, so it was also a “remainder class” in a purely logical sense. Membership seldom lasted more than three generations.

By the 1860's, the term *raznochintsy* had become a catch-all for a typically liberal and peculiarly Russian sort of bourgeoisie. With the increase of industrialization and the accelerated agrarian change after 1861, the *raznochintsy* grew in number, as did their role in society; notorious land speculators, distinguished scholars, and successful government officials emerged from the troubled waters of the group. Partly because his precarious status depended heavily on individual achievement, the *raznochinets* was often excellently educated and unusually sensitive to the moral and intellectual issues of his day; the upsurge and dominance of the *raznochinstvo* between 1861 and about 1890 was intimately connected with the efflorescence of the Russian intelligentsia. Much of the literature, particularly Gogol, Dostoyevsky and Chekhov, involves members of this class. The conflicts and variations in their pronominal usage are most illuminating.

The foregoing sketch omits many other groups and categories with a style or even a definite culture of their own. When occasion demanded, the average aristocrat would even know how to interact with the gypsies, a numerically inconsiderable but otherwise indispensable component of the Russian landscape. Substantial sections of the land were filled by partly bilingual ethnic minorities such as the Bashkir and the Kalmyks dispersed along the Volga. Entire villages of Tartars, Armenians, Finns and others were frequently encountered, and every city had its colonies of Germans, Jews and so forth, who played vital economic roles and who often emerge as minor protagonists in the literature. Along several great rivers, notably the Don and Kuban, lived almost three million Cossacks, marginal in a social, geographical and dialectal sense, but sharing many customs and values of the Great Russian peasant, such as the patriarchal households and egalitarian communes. As a privileged military caste, they were obligated to extraordinary military service, but also blessed with reduced taxes

and huge allotments of superb land. Three important novels concern Cossacks — and Cossack pronominal usage, because of slight differences from the peasantry, affords subtle insight into the underlying sentiments. Finally come the Orthodox clergy, set off by clothing, occupation, manners, dialect, and marriage patterns; although younger sons sometimes broke off and entered the *raznochinstvo*, the overwhelming majority of marriages was between the offspring of priests; the clergy was as much an endogamous caste as the gypsies.<sup>11</sup> Despite their paucity and uneven distribution, the minor groups just mentioned were known to most Russians, as were some of their patterns of pronominal usage.

Beneath its social and cultural diversity, Russian life was pervaded and determined by certain themes, all of which will be illustrated by the analyses of case material. Some of the more powerful themes were: (1) individuals tended to be aggregated into large and usually patriarchal households, and to maintain wide networks of functionally differentiated kinsmen; (2) the peasants in their communes and the workers in their coöperatives were united by economic contract and strong feelings of solidarity; (3) the ideal of fraternal loyalty and coöperation was extended to more diffuse forms of solidarity with many categories of age-mates; (4) symbols of male dominance were conjoined with the independent economic strength and psychological power of the woman; (5) class and ethnic lines, though obviously marked, were open and permitted considerable mobility; (6) ubiquitous regulations and decrees were administered by a rigidly formalistic and ostentatiously status-ridden hierarchy of officials.

Perhaps above all Russia was held together by the allegiance of everyone to the imperial authority. The draft, the poll tax and other obligations gave the peasant rights in the land which he aptly called the Tsar's or "God's". At the opposite extreme, the *dvorjanstvo* depended for rank and land on the imperial dispensation which could in principle be withdrawn at any time. Legal autocracy inhibited both peasantry and gentry from developing an absolute and traditional attachment to property such as characterized the West. The ultimate criterion of service to the Tsar and the Muscovite State provided a uniform rationale, but also made the individual emotionally subordinate to the bureaucracy in a manner that was foreign to both medieval and modern European society. Lack of rights vis-à-vis the central autocracy was connected with the lack of corporate functions in politics and government. Russia lacked true 'estates', and social action was individual and familial rather than in terms of classes. After centuries of suppression, deportations, and external control, the peasants, merchants and nobles had ceased to act jointly in the body politic.

Besides the values connected with authority in the family and the state, Russian culture of the nineteenth century may be characterized in terms of two pervasive configurations. These did not 'determine' linguistic and social symbolism, and their 'empirical' validity is precarious, to say the least; but they provide a helpful if impressionistic context for the analysis of case material that follows.

<sup>11</sup> In 1875, the clergy numbered only about 700,000 out of over seventy million.

Russia was passing very rapidly from an agrarian economy of serfs and landlords, with conservative customs, to a more industrial and Westernized way of life; the rate of industrialization during the last decades of the nineteenth century remains the fastest in European history. By 1900, Russia was still agrarian and culturally conservative in comparison with Europe, but in absolute terms it already possessed a large and restless proletariat and an unusually mobile peasantry. To such industrial growth were conjoined the economic, social and, in particular, the legal transformations connected with the truly cataclysmic Emancipation of the Serfs in 1861. The most prominent of these changes were the excessive redemption payments exacted from the peasants for their land, the rapid breakup of the large peasant households, and, finally, the shift in land ownership away from both the gentry and the communes to the advantage of *raznochintsy* speculators and an increasingly prominent type of wealthy and avaricious peasant — the *kulák*. During much of the period in question, therefore, Russia was growing prodigiously and bursting with vitality, but she was also in travail; many individuals in all social classes were forced to alter their way of life drastically. This makes the creative literature an ideal source for evidence on the deeper cognitive and emotional aspects of pronominal and social symbolism. In other words, the rapidity and disorder of economic and social change often rendered the signals of status contradictory, and all the more revealing. By the same token, the pronominal usage can give us insight into the individual experience of rapidly evolving societal and cultural systems.

Second and last, at least during the nineteenth century Russian society was dualistic to the point that many dimensions of culture involved a high tension between extreme formalism and an essential humanism. This dualism was marked even in poetry, ballet, music and painting, where a positive delight in the technicalities of obligatory conventions, often of recent foreign provenience, was combined with and even enhanced by the eruption of "elemental humanity", whether through the rigorous architecture of Tolstoy's sentences, or the last pulsations of Pavlova's *Swan Lake*. Edward Sapir was therefore right in saying:

The one thing the Russian can take seriously is elemental humanity... In personal relations we may note the curious readiness of the Russian to ignore all the institutional barriers that separate man from man... It is hard to think of the main current of Russian art as infected by the dry rot of formalism; we expect some human flash or cry to escape from behind the bars. (1924: 408).

With respect to the cultural and linguistic issue, we have noted that the strata and compartments of nineteenth century Russia were signalled in obvious and even ostentatious ways; patent distinctions of behavior, legal status and cultural norm gave to all but the majority of peasants an entirely exaggerated sense of social differentiation. The overt social formalism and "institutional barriers" stemmed in part from the universal dependence on the state autocracy, discussed above, and upon the explicitness of the bureaucratic apparatus, notably the fourteen carefully calibrated

ranks of the civil and military hierarchies: "Dans aucun état de l'Europe, le grade ou le titre de l'emploi n'a été, au même degré, la mesure de la valeur ou de la considération de l'homme" (Leroy-Beaulieu 1881: 346).

But the same Russian formalism suffered from the artificiality of most historically recent institutions; serfdom had not fully crystallized until the sixteenth century, and the systems of service and ranks had been largely created by fiat under Peter the Great. In fact, the social classes were all open in principle and were notably lacking in *esprit de corps*; the Russian notion of *soslovije* differs sharply from Western European 'class' and East Indian 'caste' because no racial, genealogical or other impassable barriers separated any one from another. Underneath the extravagant institutional barriers there throbbed a deeper sentiment, apparently shared by most individuals, that all Russians were equal in an emotional, human sense. Many of the best observers, such as Wallace, have commented on this underlying emotional egalitarianism in Russian culture. By a peculiar paradox the peasants and aristocrats could strike one outsider as "less two classes than two people superposed" (Leroy-Beaulieu 1881: 315), while at the same time others found the Russians insisting that there were no classes at all.<sup>12</sup>

The main reason for the social and even aesthetic dualism, and for the underlying sense of "elemental humanity", was that over ninety percent of the Russians were peasants, workers, Cossacks or artisans, usually living in extended households that were grouped into communes or guilds. In a behavioral sense, this meant lack of privacy and constant exposure to one's fellow man in the rough. To lower-class primitivity were adjusted a complex of attitudes, sentiments and traditions. The members of the gentry, on the other hand, were suckled, nursed and taught much of the language and folklore by peasant women. As adults, they could hardly avoid being influenced by contact with their maids, serfs and peasant concubines. The sorts of formalities dictated by the institutional paradigm were in constant interplay with the linguistic forms suggested by the sense of "elemental humanity". Cultural dualism was related to the dualism in the use of pronouns to be set forth below.

That the ten discriminations underlying pronominal usage were geared more to such basic cultural themes than to 'social behavior', is in a sense demonstrated by the facts of recent history. By the middle of the twentieth century, the regularities in village and national economy, and basic institutions such as the family and the Church, had been greatly altered. But the pronominal usage had hardly changed, precisely because it was related to communal solidarity, patriarchy, service to the autocracy, aesthetic dualism, and a few other abstract, pervasive and enduring CULTURAL patterns.<sup>13</sup> The

<sup>12</sup> A Similar generalization about dualism (but not flexibility) could be made for Spain; indeed, Spain and Russian share many generic features. This does not vitiate the relevance of the point or the realism of the over-all characterization.

<sup>13</sup> Unlike pronouns, the important status symbolism of kinship terminology was related to social behavior as much as to social culture, and did in fact change drastically in concatenation with the societal changes of the past hundred years (Friedrich 1962, 1964).



second degree abstractions of the pronominal structure were and remain causally related to second degree abstractions of the social culture.

#### DISCRIMINATIONS UNDERLYING PRONOMINAL USAGE

An important dichotomy separates the cultural systems that are explicit and understood by the native speaker from those which are primarily or exclusively understood by the analyst.

It is true that exceptionally sophisticated persons such as Leo Tolstoy were aware of all the discriminations underlying pronominal usage, and even manipulated some of them consciously. And all Russians occasionally would have to hesitate or reflect over their usage. Many of the discriminations or 'cognitive components', overlapped, and all of them could occur in some cases. Moreover, every act of usage called not only for discriminations, but for their evaluation; such evaluation intervened between the discriminations and one's pronominal response, and between one's hearing of a pronoun and one's classification of the speaker's discriminations.<sup>14</sup> But the various components and the evaluation of them that underlay pronominal usage were largely subconscious and were not conceptualized as a system by the Russian. What follows is the analyst's model.

The ten components symbolized by Russian pronominal usage were: the topic of discourse, the context of the speech event; then age, generation, sex and kinship status; then dialect, group membership and relative jural and political authority; and, finally, emotional solidarity — the sympathy and antipathy between the two speakers.

The first two discriminations are implied by all acts of speech. The speaker had mentally to associate the selection of pronouns with the TOPIC OF DISCOURSE; two officers might exchange *vy* while discussing military tactics, but revert to *ty* when chatting about women back in their quarters. A large number of such culturally defined topics — kinship, former school experiences — tended to suggest informality, whereas business and professional affairs and certain lofty (*torzhéstvennyje*) themes would encourage one to select *vy*. Of course, many subjects were relatively neutral and did not predispose the speaker in any particular direction.

Pronominal usage was also determined by the context, especially the SOCIAL CONTEXT of the speech event; an august judge would use *vy* to a tramp during proceedings in court. The social context and the topic of discourse were logically related and are often hard to separate analytically. Nonetheless, instances of clearly independent variation are not too hard to find; a daughter might use *vy* to her mother during a masked ball, but revert to *ty* when whispering about the same boy later in her mother's bedroom.

Next come four discriminations that are 'biological' in a sense, although defined in terms of the culture. Relative AGE within the same generation was not too important,

<sup>14</sup> I am indebted to Larry Krucoff for this point.

but could tip the scales between distant blood relatives, or in cases where relative authority and affection led to ambiguity. Relative GENERATION, on the other hand, was frequently decisive, as in the automatic asymmetry between any two gentry-folk relatives separated by one or more generations. Relative SEX could also decide usage in the sense that two speakers of the same sex were normally more prone to use familiar terms, whereas speakers of opposite sexes would exercise greater restraint. Finally, GENEALOGICAL DISTANCE, of which all Russians were keenly aware, provided highly specific rules and many general ones, such as that siblings of the same age at all social levels reciprocated *ty*. Age, generation, sex and genealogical distance were also important components underlying the use of Russian kinship terms.<sup>15</sup>

The third set of discriminations include various social and group phenomena. To begin with, RELATIVE AUTHORITY depended on differences in the distribution of rights and obligations; for example, the father or house chief (*bol'shák*) held many economic and legal rights, whereas the position of the son could be defined largely through his obligations in this patriarchal culture. Such jural relationships, since they involved individual statuses, could and often did vary independently of group membership. In other cases, relative authority had to be backed by the ability or readiness of the individual to exercise the raw power available; in the *meshchánstvo* household described below, the pronominal usage gradually shifted as the patriarch lost his power to beat younger relatives. Relative authority often resulted only from the resolution of conflict between several sets of dimensions — above all, between formal, bureaucratic authority and the local leadership and political values among the peasants and Cossacks. Clearly, relative authority, like most of the discriminations, implied yet other more specific jural and political dimensions.

The dimension of GROUP MEMBERSHIP primarily subsumed the notion of a household; for instance, cousins or affines of about the same age and residing in the same gentry household would exchange *ty*, which would not necessarily be true if they were living widely separated. Among the gentry, recognized affiliation to a family with a prestigious name also influenced pronominal usage. Village membership could be decisive among the peasants, as in the regions where the elders addressed each other informally but used *vy* to elders from other villages. Finally, class or caste membership determined usage; workers, peasants and Cossacks of the same age usually employed *ty* even at the outset of a conversation, whereas two gentrymen would have to begin with *vy*.

DIALECT refers to the grammatical and semantic patterns marking a particular variety of Russian. Dialect lines were not clear among many of the merchants and other transitional classes, but thick bundles of isoglosses did distinguish the gentry from the peasants, workers and Cossacks. The comparative homogeneity of the lower

<sup>15</sup> The fact that the same discriminations recur repeatedly in different semantic domains leads me to think that the total number needed for the entire semantic field would not be as high as has been suggested; as a matter of analysis, the more subsystems one analyzed, the fewer new components and logical structures one would find.

classes was incongruous with the many divergences in their way of life, but did reflect the recency of their differentiation and the constant interaction between them. The independence of dialect as a variable was demonstrated most neatly when a bilingual, blue-blooded aristocrat became so inured to *vy* that he used it even to peasant children, while receiving *ty* from their parents (Tolstoy, *Resurrection*, 218-20).

Finally comes the more psychological component of SOLIDARITY, of emotional affinity or antipathy. Close friends, lovers, and persons joined in some common purpose would use *ty*, and contrariwise, increased emotional distance between two statuses went hand-in-hand with an increased use of the formal pronoun. But 'solidarity' could subsume two quite distinct emotions such as respect and love. For example, a mutual, formal *vy* might be retained between close friends as a form of respect, while intonation and gestures signalled affection; whereas *ty* could be used to signal contempt or incipient violence or other emotions that were neither intimacy nor respect. In short, the correlation between pronouns and solidarity is complicated, and it is difficult to predict behavior within an actual system in terms of a simple continuum between the *ty* of "like-mindedness" (Brown and Gilman 1960) and the *vy* of "weak solidarity".

The level of generality of the discriminations can only be chosen with reference to one's research problem. On the one hand, in order to predict all the details and variations of usage in even one subsystem, to say nothing of the entire society, scores of concrete discriminations would have to be specified, and the number of network relations between them would be astronomical. On the other hand, Brown and Gilman's two dimensions of "power and solidarity" provide insight and predictive power at a sweeping, cross-cultural level. I have found ten variables to be both convenient and indispensable in order to account for a representative Russian sample and to predict the actual usage in supplementary texts. The ten variables are logically coördinate only in the sense that they occupy a middle ground between an abstract, bivariate level and a discrete, particularistic one. My first two variables are 'universal', implicit in all speech events. The second four are of a roughly 'biological' character, and differ enormously in their specification from culture to culture. The third set includes several dimensions of group membership. The final discrimination of 'solidarity' differs sharply from all the other nine because it relates to individual emotion and does not consist so obviously of cultural rules and principles. The relation of any variable in any given case to one or both of Brown and Gilman's macro-discriminations has been left implicit. Putting this somewhat more generally, I prefer a larger number of analytical distinctions that are only one or two steps from the data, as against only two categories that would require many intervening steps and subdivisions.

In a larger, cross-cultural sense, the ten components inferred for Russian may underlie most usage in other languages where second person pronouns are differentiated by authority and solidarity; the ten components may be universal for pronominal usage to the same significant if limited degree that Kroeber's nine components have turned out to be for kinship nomenclatures (Kroeber 1909).

Let us now turn to the types of dyadic relationship, beginning with the symmetrical use of *vy*.

#### THE DYADIC PAIRS: SYMMETRICAL *VY*

*Vy* was mandatory on certain unambiguously formal occasions. Thus, elder peasants of probity and worth (*blagomyślennyye*) would exchange the formal pronoun when meeting on the street after Sunday mass, and peasant parents and matchmakers would tend to the same usage during the formalities of negotiation and the wedding.<sup>16</sup> But such obligatory reciprocation was largely confined to non-peasants. For example, all participants used the formal pronoun in a large category of public occasions in the cities, such as a parliamentary meeting, a court session, university examinations — or, for that matter, an open, public altercation between a factory manager and leaders of the workers (Gorki, *Mother*, 134). A special poignancy could be created when, for example, former comrades-in-arms or drinking partners switched to formal terms during the challenge and execution of a duel (Lermontov, *Hero of our Time*, 192-210). While usage was thus dictated by context, the process of feedback common to all communication also meant that the usage itself symbolized an august and ceremonial atmosphere (*torzhéstvennost'*). By the same token, obligatory usage could create peculiar stresses or types of ambiguity when the speaker was conversing with a close friend, or when the defendant in the box was an idiotic peasant (Tolstoy, *Resurrection*, 32). In such cases, *vy* also signalled the artificiality of the social distance being maintained.

In the second place, *vy* was reciprocated between any two persons desiring to show respect or deference, regardless of their relative status. While such personal motivation was secondary or redundant to many socially determined usages, it also functioned independently in other cases where asymmetrical or even informal usage might have been expected, or at least permitted. Thus, at one point a student and a policeman offer to help a young woman, very drunk, who has apparently been violated or mistreated and is stumbling through the streets (Dostoyevsky, *Crime and Punishment*, 57); their sympathy and pity is partly conveyed by *vy*. In another case, a Russian officer and a surgeon of German background decide to room together in a resort town in the Caucasus. They become confidants and good friends. Their *Wahlverwandschaft* — to use Goethe's term — is congruous with a continued reciprocation of an affectionate but respectful *vy* (Lermontov, *Hero of our Time*, 200).

The role of pronominal signals for the individual's self-image is brought out in the following exchange between a proud young worker and an arrogant officer who is leading a search party (Gorki, *Mother*, 127). The officer begins with:

"This is *ty*, Andrew Nakhodka?"

"I", answered Nicholas, moving forward. Andrew stretched out his hand, took Nicholas by the shoulder, and thrust him backwards.

<sup>16</sup> In both cases, there would be a greater use of the name plus patronymic (*poimenovát' po ótchestvu*, Zlatovratsky, *Foundations*, 153).

"He made a mistake! I am Andrew!"

The officer, raising his hand and threatening Vesovshchik with his little finger, said:

"You look out with me!"

He began to dig in his papers.

"Nakhodka, were *ty* ever brought before an inquiry on political grounds?" he asked.

"I was summoned in Rostov, and in Saratov ... only there the policeman said *vy* to me."

The officer winked his right eye, wiped it and, baring his small teeth, began to say:

"But isn't it known to *vy*, Nakhodka, precisely to *vy*, just who are the rascals who are distributing criminal proclamations in the factory, aah?"

Here we see a factory worker insisting — with admittedly ambiguous success — that an inspecting officer show him the proper personal respect.

This and other cases in my data point to important discriminations not covered by my system: those of sarcasm and irony. Under certain circumstances the opposite of the expected usage could confuse, humiliate or affront an addressee. Such inverse usage was especially devastating before a group of people who were more aware even than the victim of the disparity between the verbal symbolism and the underlying social realities, as illustrated by the following passage from the twentieth chapter of Tolstoy's *Childhood*: "Grandmother had a singular gift of expressing her opinion about people under certain circumstances by using the plural and singular pronouns of the second person together with a certain tone of voice. She used *vy* and *ty* contrary to general custom, and on her lips the shades of meaning acquired an entirely different significance. When a young prince walked up to her she said a few words calling him *vy* and looked at him with an expression of such contempt that if I had been in his place I would have become utterly confused..." (An ancient countess would normally use *ty* to a young prince.) In contrast to sarcasm, humor does not appear to have been significantly related to pronominal usage.

We have discussed formal occasions and the particular needs to show or receive deference. *Vy*, in the third place, was determined by a large set of contexts that can be defined exclusively through the relative status of the participants within the larger context of the society. The higher the individual in the social system, the more he tended to both receive and use *vy* with persons of lower standing, including lackeys (Tolstoy, *Resurrection*, 18, 100). Such usage to almost everyone except one's siblings and spouse was largely a carry-over from the patterns of the French high aristocracy, probably the most formal in Europe. Such bilingual blue-bloods lived in what approximated a *vy*-universe.<sup>17</sup>

Relative status within the larger society was also symbolized by automatic formality on first encounter or casual acquaintance between all officials and gentry who were not lovers, close friends, or certain types of kin. In one novel a petty official reciprocates *vy* with a long series of rustic landlords with whom he is often cordial or even pseudo-intimate (Gogol, *Dead Souls*, 102-3). Russian officers and non-coms usually stayed on

<sup>17</sup> Brown and Gilman, writing in 1960, reported a French aristocrat who could remember using *tu* only to an old woman who had been his nurse (1960: 270).

formal terms, just as did gentry with the tutors and governesses of their children, and also prison officials with political prisoners of the educated classes. Moving yet further down the ladder, a wealthy peasant would normally exchange *vy* with older peasants in the village he was visiting (Zlatovratsky, *Foundations*, 459), and elders from neighboring villages might do the same under certain circumstances. In all the instances just discussed, we find formality between persons who do not know each other well and who are of somehow coördinate social status.

Among the gentry, at least, the formal pronoun was enjoined between certain categories of relatives and potential relatives, such as a man and the consanguines of a girl he was wooing (Dostoyevsky, *Crime and Punishment*, 218; Gorki, *Childhood*, 168), between cousins who were strangers, and between parents-in-law and their children-in-law. Man and woman held to *vy* during a formal courtship until entering upon marriage, or sexual relations, or some comparably intense experience. Women tended to use *vy* more than men, among the gentry because of their greater concern for propriety, among all classes because of their partially subordinate status. In one intriguing case that cuts across class lines, two women held to the formal term while they passed through three relative statuses: landlord's niece as against the same landlord's maid, provincial actress as against the landlord's concubine, and, finally, a broken prostitute as against the village wanton (Saltykov-Shchedrin, *Golovlyov Family*, 265, 289). The persisting use of *vy* despite considerable emotional affinity and, toward the end, approximately equal power, lends unique flavor to this evolving bond — social status proving dominant over both personal feelings and the general situation.

#### THE ASYMMETRICAL RELATIONSHIP

In the patriarchal society of Russia deference and obedience were widely accorded to age. Among the gentry, a child would reciprocate *ty* with the mother until about school age, but older children and adolescents used the formal pronoun to their parents. These patterns did not hold for some aristocratic families, and there may well have been marked differences between parents, with less formality toward the mother, particularly between mother and daughter. Similarly asymmetrical were the ties between married daughters and either parent, and between such distant consanguines as a man and his cousin's son's wife (Aksakov, *Family Chronicle*, 82, 157, 188). Step-relatives and affines separated by one or more generations were also on *vy/ty* terms. Two concrete examples must suffice for genealogical distance. In one case, an adult prince received *ty* from an aunt thirty years his senior, although both were exchanging the French *vous* (Tolstoy, *Resurrection*, 257, 260, 270). In another, singular instance, an elderly landlord and his niece retained the asymmetrical terms even after he had become a psychotic and disreputable miser and she had returned home from a career as an actress and prostitute, broken through drink and tuberculosis (Saltykov-Shchedrin, *Golovlyov Family*, 181, 267). At one point the miser even

suggests that his niece become his mistress. Among the gentry and wealthier merchant classes, then, usage was necessarily asymmetrical between relatives of any sort, including those by adoption, who were separated by one or more generations.

Among peasants and Cossacks, asymmetrical usage was occasional or regional, but was not generally obligatory between the household head and his younger housemates, with one major exception: in most *meshchánstvo* households and in the larger peasant households of the central 'industrial' zones, the *bol'shák*, or household head, normally received *vy* and addressed all as *ty*. These were precisely the two social groups and the geographical area where Great Russian patriarchy reached its acme, and where the influence of the *vy*-using gentry was most deeply felt. Asymmetrical usage also set off a peasant from his parents-in-law, and often, a girl from her father-in-law. In sum, asymmetrical usage tended to emerge between two peasants when they were separated by two or more components, such as generation, community, extreme authority, or the solemnity of the situation.

Superior and inferior authority within the established officialdom was pronominally symbolized in the relation between teacher and pupils, law officers and criminal prisoners, and officers or non-commissioned officers with their soldiers; the last of these three usages was congruous with the gentry or middle class origin of most officers, and the proletarian or peasant background of most soldiers. The Cossacks, despite their strong egalitarianism, reflected their common experience as members of a military caste by scrupulously using *vy* to officers, non-coms, gentry, and all government officials.<sup>18</sup>

One well-known short-story of Pushkin ("The Stationmaster") illustrates how pronouns could symbolize authority. A young hussar feigns illness while travelling in order to seduce the adolescent daughter of a fifty-year-old stationmaster and war veteran. The broken-hearted father makes his way into the large house in the capital where his daughter is being kept as a mistress, but during a final, climactic scene is literally thrown out by the irate young aristocrat. Neither man ever deviates from asymmetrical usage. Thus, a low-ranking official could be subordinate to any member of the high aristocracy. The frequency with which social differences overrode age demonstrates the strength of class cleavage in the first part of the century.

Perhaps of greater psychological interest than the school, prison, army, and officialdom were the domains such as the gentry household and the landed estate where asymmetry was enjoined and often automatically ascribed for the members of an entire group. Any patriarch or matriarch with the abilities and motivation to realize his or her power would eventually use *ty* to all, and receive *vy* from all except the spouse (Saltykov-Shchedrin, *Golovlyov Family*, 6, 15, 71), and sometimes even from the spouse; such gentry patterns, emphasizing the formal or secular aspect of the father's or husband's authority, became models for the *meshchánstvo* and peasant households mentioned above. As a rule, landlords and their estate managers were

<sup>18</sup> One of the first acts of the Provisional Government in 1917 was to force all commissioned officers to use *vy* to privates; but the Red Army reintroduced *ty*, eventually extending it to all subordinates.

on *vy/ty* terms, although both kinds of symmetrical relationship might occur for personal reasons. Country squires usually remained on a *vy/ty* basis with their peasant concubines, although genuine affection and isolation could lead to a mutual *ty* within the rustic privacy of the home.

Pronominal usage between peasants and other classes entailed surprises. For example, a wealthy, respectable and urbanized serf in Moscow would still use *vy* and receive *ty* from the dissolute and disgraced son of his distant proprietor, ascribed status taking precedent over both age and actual power (Saltykov-Shchedrin, *Golovlyov Family*, 21-9). A second case comes from the *Childhood* of Tolstoy, always so sensitive to status: a fully mature huntsman and house serf is pronominally subordinate to the little son of his landlord (35). A third instance comes from the same source. As a boy, Tolstoy was cared for by a beloved elderly peasant woman who, incidentally, was on *vy/vy* terms with his mother. At one point this nurse scolded him, saying, "Don't *ty* soil the tablecloth." Little Tolstoy gets furious and exclaims, sputtering, "How ... Natalja is saying *ty* to me..." She apologizes with, "That's enough, daddy (*bátjushka*), don't *vy* cry..." at which he is comforted (54). Here a small boy is literally calling an elderly woman into line on her usage. Such specific functions of the pronoun as a symbol of highly personal feelings renders difficult predictions through broad categories of 'power and solidarity'.

Other obvious differences in social rather than administrative status could determine usage. With the exceptions already noted, all persons used *ty* to small children and would use *ty* and expect *vy* with pre-adolescent children. Any member of the gentry tended to use *ty* to a lower-class servant, typically the valet or lackey, or the waiter in a tavern. The strength of the combination of age and class differences is shown by a gentleman-landlord and his peasant boarder, who automatically entered upon an asymmetrical relationship and held to it even when the youth was demanding the return of a large sum that the old codger had borrowed and squandered (Zlatovratsky, *Foundations*, 288, 350). *Ty* was used downward to the members of minority groups whose poverty and ethnic status were clear, as between a middle-aged landlord and a Jewish watchman (Dostoyevsky, *Crime and Punishment*, 544-5). Any person with education and with civil or military rank would immediately enter into *vy/ty* terms with some irregular semi-legal persons, such as a professional thief, a gypsy, a beggar, or a criminal. In sum, asymmetrical relationships were automatic when social differences were clear and mutually felt.

#### THE SYMMETRICAL *TY*

Within and between all social classes children under about eleven or twelve exchanged *ty*. In one delightful scene, an aloof, isolated and already psychotic eight-year-old is approached by three adventurous, arrow-shooting little "Mohicans" who are also on vacation, and immediately begin using *ty* (Sologub, "In bondage", 241-3). The



apparent assumption was that children were essentially equal and potentially solidary. In other words, the status of child overwhelmed or obscured that of class. Childhood informality also created the frequent comradeship between the offspring of peasants and landlords, and contributed to the peculiar nostalgia for an irretrievable past that was felt by so many of the gentry in their adult years.<sup>19</sup>

Second, the informal pronoun was exchanged with certain non-humans, such as God, the Devil, demons, and other objects, animals, spirits and essences that might be addressed; the folklore and the texts containing fantasy show that the usage was returned. The conversations between Gorki's grandmother and God reveal the intimacy of such relations with non-humans and the supernatural.

In the third place, all members of the lower classes strongly tended to reciprocate *ty*, even among total strangers. In one case, a proletarian woman exchanges the informal pronoun with an elder peasant from another community, whereas her son and the same man are on formal terms. In a second instance, a wealthy *kulák* and his impoverished body servant express through *ty* their sense of common peasant origin and their personal affinity (Zlatovratsky, *Foundations*, 520). In some northern, northeastern and Siberian regions, lacking landlords and far from the urban centers, *ty* was probably the only functional pronoun.<sup>20</sup>

Membership in the same community was often decisive. Thus relations — irrespective of age — were mutually informal between proletarian neighbors in the squalid factory towns (Gorki, *Mother*, 151). The villages of Cossacks and Great Russian peasants were pronominal in-groups of a sort, the members conceiving of each other as relatives and addressing even the village elder informally (Zlatovratsky, *Foundations*, 455). In one revealing case, the peasants in a small commune start using formal terms with a young visiting Muscovite; but after they have learned of his friendship with a local youth, their whole attitude and their pronominal usage shift, as is summed up by one matron: "So *ty*, my dear (kinsman) are on friendly terms with him" (Zlatovratsky, *Foundations*, 153).

Until the middle of the century, peasants usually said *ty* to the landlord, but not to his wife; this was part of their tendency toward informality but, in a perhaps more important sense, it symbolized their conceptual grouping of persons of supreme authority; the landlord's rights over his serfs included purchase and marital arrangements. With the partial exceptions noted above, the household chief, the landlord, Tsar and God were all addressed with *ty* and the quasi-kinship term *bátjushka* "little father". Thus a striking feature of authority in Russia as against the West was that *vy* generally did symbolize greater power, but that when the greatness passed a certain point the speaker switched back to what might be called the *ty* of total subordination or of

<sup>19</sup> It is psychologically intriguing that the *ty* was primarily associated with infant experiences and corresponds to the combination of the so-called "ego-id", with its "devotion to the pleasure principle" (Silverberg 1940: 511); by the same psychiatric token, *ty* would be a symbol of intimate personal identity (especially to the aristocracy), whereas *vy* (especially to the lower classes), would correspond somehow to the superego.

<sup>20</sup> I am indebted to Howard Aronson for this point.

an intimacy that could not be jeopardized.<sup>21</sup> From another point of view, *ty* to God, Tsar and squire emphasized the fatherly aspect of their jural authority.

In addition to the social categories just discussed, pan-Russian patterns enjoined a mutual *ty* between brothers and sisters. This was related to the norms of coöperation and loyalty between brothers and, in addition, was extended to many kinds of comrades and associates. Peasants, Cossacks, and artisans banded together into brotherhoods, and there was considerable overlap both in sentiment and usage between the words for commune (*mir*) and coöperative (*artél'*). In addition, ritual brotherhood, sealed by exchanging crosses worn around the neck, was important at all levels. Students, officers, and other members of the educated classes had a ritual of "drinking to *ty*", but this foreign *Bruderschaft* appears to have been rare in the last century and is not evidenced in my texts. In any case, fraternal solidarity of many kinds was a pervasive and integrating theme in Russian culture and was partly symbolized by the reciprocal *ty*.

The informal pronoun was usual between spouses at all social levels, as would be congruous with the balance of rights between man and woman that underlay the symbols of male dominance. In many merchant families and lower-middle class *meshchánstvo* families, however, the wives addressed their husbands with *vy* along with the given name and patronymic. Among the bilingual gentry the mutual informality ran counter to pressures toward a reciprocal *vy* on the model of French.<sup>22</sup> And conjugal usage could be poignantly ambiguous, as when estranged or even hostile spouses persisted with *ty*. Even among the high aristocracy, lovers held to informal terms long after the liaison had cooled (Lermontov, *Hero of our Time*, 141).

Aside from siblings and spouses, *ty* was reciprocated between more distant kinsmen under certain conditions; for example, gentry cousins who knew each other reasonably well would be on informal terms. Second degree relatives through marriage, such as the spouse's siblings or the sibling's spouses, would usually reciprocate *ty* with a speaker of about the same age, although this could produce conflicts if they were personally antipathetic; in one case, a man and his sister's husband "tried to use *ty*" after the sister's marriage, "but remained on *vy* terms" (Tolstoy, *Resurrection*, 332). In general, usage between affines varied considerably. As a final point, the growth of the individual at all social levels was critically affected by the grandmother, who was at once a nurse, a story-teller, and a repository of norms and traditions; she varied from the peasant *bábushka* of Gorki's *Childhood* to the august and matriarchal *bábushka* of Leo Tolstoy. Throughout life most Russians remained on informal terms with at least one grandmother, although among the high aristocracy she might be addressed with *vy*, while a peasant nurse filled many of the more intimate and homely roles.

<sup>21</sup> As Julian Pitt-Rivers points out, total subordination implies intimacy, whereas formal social usage implies social distance which obtains where respect might conceivably be denied.

<sup>22</sup> Indeed, one of the most fascinating problems, barely hinted at in this paper, is the degree of congruity between French and Russian usage; both languages functioned as largely independent systems for classifying the same set of actual and potential addressees.

The informal pronoun was also reciprocated between members of the gentry who had been joined through shared experiences, such as regular participation in card games or a common revolutionary ideology. Classmates from the same secondary school and the alumni of the same privileged school, such as the Corps of Pages, were formally linked by *ty*. As a rule, men who had moved in the same circles as university students would continue with *ty* throughout life, although conflicts could arise if their statuses diverged too much. In one case, a prince and an old army comrade use *ty* although the former is embarrassed by the implied closeness he no longer feels (Tolstoy, *Resurrection*, 245-46); reunion with an old chum might entail pronominal *gaucherie*.<sup>23</sup> The informal usage between friends and students generally expressed a certain affection or *esprit de corps*, but at a more profound level it was the felt extension of a kinship bond — just as, conversely, the use of *vy* within the peasant village, which was an extended kinship group, was often associated with the introduction of external standards by the governmental bureaucracy.

#### THE DYNAMICS OF PRONOMINAL USAGE: EIGHT CASES OF 'SWITCHING' AND 'BREAKTHROUGH'

Crucial to linguistics and social anthropology is the distinction between studies of synchronic systems that have considerable constancy over at least one generation, as against the study of historical sequences and cultural evolution. But despite the value of separating the two approaches to some extent, a rigid dichotomy can only be maintained at the cost of the light which diachronic information sheds on the synchronic system; by following replacement, loss and rearrangement through time, the dynamic relations of units can be inferred.

In addition to historical and evolutionary change, any synchronic system shows alternation, variation and kinds of option which can lead to additional inferences about functional and hierarchical relations. For example, the numerous cases of dramatic, rapid and often erratic switching and 'pronominal breakthrough' often suggest the hierarchical relations between the discriminations. In other words, just as 'regular usage' symbolized a sufficient and necessary co-occurrence of discriminations, so the many cases of switching symbolized some realignment, or a change in relative power, or simply the addition or subtraction of a component. This should help us to ferret out both the constants within one system, and the dynamic relations between those constants. Let us review eight extended cases of pronominal 'switching' and 'breakthrough' with the aim of indicating and illustrating some of the more obvious interrelationships between components.

<sup>23</sup> This contrasts with many other languages, such as German, where *du* to a former classmate is clearly not felt as necessarily implying anything more than the former group membership.

*Comradeship between Pechorin and Maxim Maximych in  
Lermontov's "A Hero of Our Time"*

An arrogant, aristocratic lieutenant and a grizzled, older captain find themselves thrust together as the only officers on an isolated outpost in the Caucasus. Reciprocal formality at first seems appropriate to both. But while the latter is sitting on the young lieutenant's bed and discussing a confidential matter, he switches to *ty*. When the lieutenant appears to suggest insubordination, however, the captain reverts to *vy* as he issues a peremptory demand; for the older man, the power of military rank is in delicate balance with the emotions uniting the two lonely comrades in the Caucasus.

In the succeeding scenes the lieutenant uses *vy* at all times — partly because he is almost thirty years younger, partly because he comes from the high aristocracy, partly because (as is borne out by several episodes) he is emotionally incapable of warm, positive attachments to other people; as is so often the case, the relative power of the three determinants cannot be ascertained. On the other hand, I would deduce that in some contexts the young lieutenant would have used *ty* or that at least after their separation the older man thought of him as employing the more intimate pronoun. During the final scene the two friends accidentally meet again at an inn. The affectionate and simple-hearted captain rushes forward impulsively, but is countered with, "How delighted I am, dear Maxim Maximych! Well, how are *vy*?"

"But...*ty*...*vy*?" muttered the old man with tears in his eyes" (88).

The wrenching quality of the passage turns on the binary and here ironic choice between two pronouns.

*The Meshchánstvo Household of Gorki's "Childhood"*

Gorki portrays a household ridden by animosity and suspicion between "stark human beings existing primarily in and for themselves" (Sapir 1924: 408). The elderly patriarch rules with despotic hand over wife, progeny, and servants, using *ty* to all and usually receiving the *vy* of respect or subservience.

Of the children, one daughter has been partly separated from the extended group by marriage into a more cultivated and socially sophisticated sphere; she regularly returns *ty* to the old man. After the death of her husband, she is temporarily reincorporated. There are many open conflicts. At one point she switches to *vy* when erupting in protest against her father, the patriarch, who is preparing for her little son (Maxim Gorki) one of the sadistic canings that almost seem rituals of intensification (27). Later, as the family is trying to marry her off to a man she dislikes, she again switches to *vy* partly in defiance of her father (162). Both times, the shift from *ty* to *vy* by a young woman speaking to the old patriarch serves to express her personal resentment and rebellion, and to project her superior, achieved status onto the foreground. Several exchanges between the patriarch and his eldest son also illustrate the hostile switching to *vy* within the household.

Gorki's *bábushka* tends to use *vy* to her husband, the patriarch. But during extreme crises, such as the great and ruinous fire (61-3), she switches to *ty*, and later, as he is losing his property and relatives, and sinking into abject poverty and weakness, she uses nothing but the informal pronoun. In sum, long term interaction within the same group shows how one member could erupt with *vy* and a second with *ty* when speaking to the same authority figure during a crisis.

*A Proletarian Mother, Her Son, and Her Son's Friends*

After the death of her brutal husband, a mother somewhere in her forties is left with an adolescent son who soon turns to private study and underground revolutionary activities (Gorki, *Mother*). More and more books appear on his shelves and, to signal his growing cultivation, "... he said *vy* to her and called her *mamásha*, but sometimes, suddenly, he would address her tenderly:

'*Ty*, mother, please don't worry. I will be coming home late...' (113). And later: "He said 'mother' and '*ty*' to her, as was his wont only when he drew closer to her..."

About the time of the conversations just described, the son brings home a worker about twenty-eight years old to live with the family. At first the mother calls him *vy* along with the respectful name plus patronymic (Andrei Anisimovich), plus the respectful and affectionate kinship term that literally means "dear little father" (*bátjushka*). But they grow more intimate as the weeks pass, as is clear from the following scene:

"Andrjusha, *vy* ought to mend your shoes, or *vy* will catch cold." To which he responds:

"*Vy* are almost like a true mother to me" (*a, mózhet, vy i est' rodnája mat' mojá*). They remain on a formal basis, while exchanging the 'kinship terms', *bátjushka* and *nénka*. But after the young friend confesses to the murder of a political informer (166, 168), the mother switches to *ty* although he continues with *vy*. At this point her son actually comments on the pronominal usage:

"'*Ty* know, *ty* did well, that *ty* began to say '*ty*' to him after that.' She looked at him with surprise and said, 'Yes, but I didn't even notice how it happened! He has become so close to me — I can't say how!'"

The mother's relations to her son's other revolutionary friends were generally formal, even in the case of women such as her son's sweetheart, whom she got to know rather well. She remained on formal terms with an educated school teacher though he lived in her house and became emotionally very close (247). The younger workers' use of *vy* to her actually became a source of pride: "It pleased her that this young man, the outstanding brawler in the neighborhood, would speak to her secretly, using *vy*..." (137). The main exception to such reciprocal formality was the relation between the mother and a young revolutionary in his thirties before the hour of his death, when she becomes a loving nurse to him and he is a hero to her (201). But I hasten to add that even in such critical relations between two proletarians fused by a common ideology, one could not predict a switch to *ty* with absolute certainty.

*Chichikov and Korobochka in Gogol's "Dead Souls"*

Chichikov, a well-bred petty official, gets lost at night in a snowstorm, and finally knocks on the door of the unknown proprietress of a small estate. Both he and the maid at first use *ty*, reflecting their mutual surprise and the spontaneity of the situation. But after hearing a few words, she switches her pronouns, apparently in reaction to his urban accent: "But who are *vy*?"

Later that night Chichikov and the proprietress reciprocate *vy* as they converse over tea, although repeatedly interjecting the kinship terms, *mátushka* (derived from "mother"), and *bátjushka*. The proprietress switches to the informal *ty* when expressing sympathy over his carriage accident, and when bidding him goodnight she calls him not only *bátjushka* but literally "my father" (*moj otéts*), and tenderly offers to scratch his heels, a soporific comfort her deceased husband had habitually required (34). By this time both Chichikov and his hostess feel themselves to be approximately on a par.

The following day finds our guest, who is about forty, addressing his hostess in familiar terms; because of her seemingly small estate, "he didn't stand at all on ceremony". He soon asks her last name, and is told, "Korobochka, (the wife of) a collegiate secretary", and learns on further inquiry that her name and patronymic is Nastásja Petrovna. The official title impresses Chichikov, and from then on he is careful to address her respectfully. He begins to importune her to sell him the title to such of her serfs as have died since the last census. As soon as this extraordinary intention becomes clear, Korobochka switches from *vy* to *ty*. By this time the asymmetrical relationship (37-42) is coming to symbolize their mutual and increasingly clear-cut awareness that she is an established owner of serfs and land, a true *poméshchitsa*, whereas he is materializing into a rather suspicious sort of solicitor. The function of *ty* as an expression of Korobochka's growing wariness comes out in several strongly felt utterances: "What are they (the dead souls) to *ty*?" or "Are *ty* deceiving me, *bátjushka*?" or "What terrible things *ty* say!" The unequal quality of the relationship is underscored by his combination of *vy* plus Nastasja Petrovna, whereas she mentions no proper name at all (partly because he had dodged her original question about it). But both continue to interlard kinship terms.

Chichikov finally persuades Korobochka to sell her dead souls by implying that he is taking government contracts and may be in a position to buy some of her vegetables. In the last scene, after a rich dinner for the "contractor", she switches to *vy* when asking if he will buy some pork lard, but then goes right back to *ty* when discussing his carriage and road conditions (41).

In ten of the most masterful pages in *Dead Souls* the dyadic relationship evolves from *vy/vy*, to *ty/vy* to *vy/vy* to *ty/vy* to *vy/vy*, and finally to *ty/vy*. This illustrates the frequency and rapidity of pronominal switching as total strangers adjusted to each other. More particularly, it illustrates the complexity of covariation between pronouns and various sorts of "kinship terms". Thus, *ty* plus a kinship term of address occurred quite normally among the gentry. But less redundant and more interesting was the

conjunction of the formal pronoun with a kinship term such as *mátushka*, enabling the speaker such as Chichikov to partially cross the gap between himself and his interlocutor.

*The Prince and the Prostitute in "Resurrection"*

The heroine of Tolstoy's novel, *Resurrection*, was born the daughter of a wandering gypsy and the village wanton. She was then adopted and reared in the cultivated home of an elderly, aristocratic lady. When the eighteen-year-old nephew of her patroness comes home from school on vacation, he unselfconsciously starts out using *ty*. She responds with the respectful *vy* (47), except when he steals a kiss during a game, and even then her *vot tebé raz* is part of a fairly frozen, exclamatory expression. The contrast between their asymmetrical address comes out neatly when he is leaving for the university and they bid farewell: (*Ty*) *Proshcháj, Katjúsha*, as against (*Vy*) *Proshchájte, Dmítryj Ivánovich*. During their first, idyllic acquaintance their usage reflects caste status more or less as one would predict.

Three years later the prince returns as a pleasure-seeking officer. She still uses *vy* (54), whereas he vacillates before finally settling on the same form. The young people are soon in love, but continue on formal terms; when they meet after the moving Easter service, their usage is apparently reinforced by the solemnity of the occasion (60). But a few hours later on the same Sunday he switches to *ty* while trying to embrace her in a corridor, and this is conjoined with the diminutive of her first name. She sticks to *vy* and the congruously respectful first name plus patronymic, as above. During the physical seduction that evening he repeatedly says *ty*, whereas she says *vy* while her eyes say *ty*: "I am yours (*tvojá*)". The physical conquest of a woman appears to have been inseparably connected with a switch to *ty* by the man at least; the woman might use *vy*, but her gestures, intonation and so forth indicated that she was thinking and feeling *ty*.

Twelve years later the same prince finds himself in the juror's box, asked to pass judgement on an attractive young prostitute who has been accused of poisoning and robbing a client. He recognizes her as the village girl he seduced long ago. Feeling remorse about his role in precipitating her downfall, he visits her later in jail. As he begins to speak (153) he uses *ty*, possibly in recognition of the enormous social distance between them, possibly to express his impulsive love. But following an interruption in the conversation, he reverts to *vy* as part of the desire to increase the propriety of their relationship.

Soon afterwards he returns to the jail and declares his resolve to marry her. At this her instant reaction is to recall the degrading scene the morning after the seduction when he tried to thrust one hundred rubles into her hand. With a sort of ironic reference she erupts with: "There is your (*tvojá*) price." The prince also switches pronouns with, "I won't leave *tebjá*!" For a brief period thereafter she reiterates a *ty* of anger and contempt: "*Ty* are a prince ... (*ty*) go away!" (173-4). But after a second brief interruption by the warden she returns to *vy* although the prince con-

tinues with *ty*, as in “*Ty* don’t believe me.” As the interview draws to a close both interlocutors grow colder, and he finally switches back to *vy* himself, saying, “*Vy* think it over.” Here we see the pronoun functioning as a sort of exclamatory particle, a two-phoneme signal of rapid emotional shifts.

Shortly afterwards, Katja is convicted. When the ill-matched couple meet again they at first reciprocate *vy*, but when he proposes to her for the second time, it is with, “I am asking *tebjá* to marry me.” She rejects him using the formal pronoun, and he almost immediately switches back to *vy* himself. Eight pages later, when he dreams of her, she says, “I am a female convict, but *vy* are a prince.”

Later the prince returns from St. Petersburg after an unsuccessful attempt to appeal her sentence. He is chagrined to learn that she has been accused of gratifying one of the sergeants and as a result has been fired from an advantageous job in the prison hospital. During the ensuing dialogue he feels ambivalent, and Katja is mortified. Both use *vy* and never deviate from it for the rest of the story, during their long trek to Siberia (357, 406), nor at their last meeting (453), when her moral redemption has been largely achieved through the influence of the political prisoners, to one of whom, a psychotic terrorist, she is already promised in marriage.

The particular value of the *Resurrection* lies in the interaction between persons from opposite ends of the social continuum. In Prague School terms, at either end of the continuum one of the two pronouns was unmarked, while the other was marked and conveyed more precise information. Thus, for a prince *vy* was the more frequent and the less marked pronoun, whereas for a prostitute *ty* was comparatively unmarked; in either case a switch to or a maintenance of the marked pronoun implied special hostility, affection or ambivalence.<sup>24</sup>

*The Denouement between Dunja and Svidrigailov in  
“Crime and Punishment”*

Svidrigailov, an extremely intelligent but also psychopathic landlord, has fallen hopelessly in love with a young woman. They had already known one another as neighbors in the country, and when they meet again in Moscow, *vy* is reciprocated, as would be normal for two gentry during the initial stages of courtship. Eventually he lures her into his apartment, but stays with the formal pronoun even when declaring his love: “I love you endlessly” (*ja vas beskonéchno ljubljú*). She tries to escape, but finds the door locked. He says, “*Vy* are agitating yourself in vain.” At this point, because she has grown excited, Dunja switches to *ty*: “(*Ty*) open the door immediately, immediately, base man ... so this (means) rape!”

But Svidrigailov continues with *vy* while explaining that, since no one is near them, she would never be blamed for surrendering to his advances and, in addition, that no one would ever believe her story about rape. On the other hand, by submitting to his wishes and marrying him, she can help to free her brother from an accusation of murder.

<sup>24</sup> I am grateful to Milka Ivić for this point.



At this point, Dunja draws a miniature revolver (528). He exclaims, still using *vy*. Dunja gets frenzied, and accuses him of having poisoned his wife: "*Ty!* *Ty* hinted it to me yourself; *ty* spoke to me of the poison... I know, *ty* made a trip to get it... *ty* had it ready... it was *ty*... it was decidedly *ty*, lout!" The pronoun is repeated seven times in two and a half lines of Russian, showing its function as an exclamation and an expression of feeling about the addressee.

After being called *ty* seven times, Svidrigailov, as perhaps might be expected, also switches to the informal pronoun, saying, "Even if it were true, *ty* were the reason." They reciprocate the informal pronoun while she is shooting at him from six paces, but he goes back again to *vy* while scoffing at the mere graze on his scalp, and inviting her to try again at half the distance. The second time her pistol misfires. She tosses it away. Svidrigailov puts his arm around her waist. Her body is trembling.

"(*Ty*) let me go", she says, imploring.

At this juncture, Dostoyevsky himself steps in to comment on their usage: "The *ty* was already not being pronounced as it had been a moment before" (*éto 'ty' uzhé kak-to ne tak progovorenó, kak dáveshnee*),<sup>25</sup> presumably meaning that Dunja's intonation signals an antipathy that Svidrigailov will never be able to bridge. Switching to *ty* he quietly asks, "So *ty* don't love me?" She shakes her head in negation. "And ... *ty* cannot ... never?" he whispers desperately.

"Never!" answers Dunja.

Seconds (?) later Svidrigailov releases her, stands for a moment by the window, then hands her the keys, saying, "(*Vy*) take them, go away fast!" And the author adds that "there was a terrible note in the last word." Shortly afterwards Svidrigailov commits suicide.

The brief but tragic denouement covers only four pages, during which the pronominal usage shifts four times, illustrating how the bond between two human beings could change from minute to minute and even from second to second. From another viewpoint, it shows a rapid alternation between an explicit and an implicit *ty* relationship. The sequence suggests that a man would shift to *ty* first when reaching out for the love of a woman, although reverting to *vy* when the topic was not love itself. The woman, on the other hand, might switch to *ty* as an expression of terror, or contempt, or some other underlying antipathy. The failure of either individual to once use a personal, proper name enhances the abstract, universal atmosphere of the scene.

#### *Raskolnikov in "Crime and Punishment"*

Raskolnikov, an undernourished and idealistic student in his early twenties, is obsessed with the theoretical implications of wilfully breaking a fundamental moral law. At the outset the relations between the young student and his intended victim, an old usurer, are formal; he calls her by her name and patronymic and sometimes even adds

<sup>25</sup> Dostoyevsky thereby also implies that the pronoun itself was expressed through the verbal form alone (*otpusť* in this case), even though it was not actually pronounced.

the extremely deferential *-s* particle. But when he has come to kill her, with an ax tucked under his coat, and stands glaring at her, she switches to *ty* and then instantly back to *vy* (86), presumably in response to her intimations of dangers.

The relationship between Raskolnikov and the investigating official, so deservedly famous on aesthetic grounds, also sheds unusual light on pronominal usage. During the first examination they reciprocate *vy* (269, 284), but when the accused breaks down during a long interrogation, he erupts with, "*Ty* are lying!" symbolizing his personal disorganization. As soon as his agitation has begun to subside, and the interview is drawing to a close, he switches to *vy* and apologizes for his behavior (376). The investigator, who has never varied from the formal pronoun, concludes by thinking in terms of *ty*, as one would expect him to do toward a man whom he has broken with his questions. Like Tolstoy, the author provides an invaluable dimension by telling us what pronoun people are THINKING or FEELING as well as what they actually say.

### *Raskolnikov and Sonja*

Raskolnikov is living in a small garret that adjoins a room filled with two half-crazed parents and their hungry children. One daughter, a shy, nervous girl of about eighteen, has turned as inconspicuously as possible to prostitution in order to help support her impoverished but originally upper-class family.

These two young people reciprocate *vy* during their casual first contacts. But after his brutal ax-murder of the old usurer and her niece, Raskolnikov is torn by guilt and needs a confidant. He goes to visit the girl in her lodgings. As the conversation develops, they increasingly sense their need for each other. His voice becomes "quiet and tender". But Sonja remains formal while talking about the suffering of her family, and even of her first professional experiences; once she affixes the hyper-respectful particle, *-s*. As they continue to draw closer, Raskolnikov realizes with more terrible clarity the conflict between her degrading profession and her innocent, generous nature. He falls to his knees and kisses her feet (*nógi*). Sonja cries out, "What are *vy* doing?" (precisely, *chto vy, chto vy éto! Perédo mnoj!*), and Raskolnikov immediately rises and answers, "I did not bow down to *ty*, I bowed down to all human suffering" (343). This scene, deservedly one of the most renowned in all Russian literature, owes its drama in no small measure to the profound, kaleidoscopic reorganization within Raskolnikov's mind; Sonja is elevated to a vast symbol, an abstract, metaphysical idea, and her shift in status is signalled at the critical moment by a switch in pronouns.

But let us continue. The asymmetrical relationship is held during a subsequent discussion and while Sonja is reading the story of Lazarus: "'I am the resurrection and the life'... The stub was already burning low in the candleholder, murkily lighting up in a beggarly room the murderer and the prostitute who had been strangely united through a reading of the Holy Book" (350). Raskolnikov, realizing that Sonja is also half insane, asks her to go with him "along the same road", and announces that he

has chosen to tell her who murdered the niece of the usurer. Sonja, terrified by his wild-eyed conduct, and not understanding his motives, persists with *vy*, and so they part.

Shortly afterwards, Sonja is accused of theft in a humiliating scene, but is stoutly defended by Raskolnikov. He again visits her lodgings, and they start out reciprocating *vy*. Their relationship soon grows very tense as Raskolnikov poses a terrible if hypothetical moral question, and Sonja breaks down, sobbing and appealing to Providence: "Did you indeed come only to torture?" At this Raskolnikov, feeling a burst of remorse, switches his pronouns, while answering, "But of course *ty* are right, Sonja" (*a ved' ty pravá*). His voice "weakens" and his entire manner alters as he confesses that he has come to ask her forgiveness. They sit together on her bed and he gradually begins his confession, at first by giving her hints and asking her to guess. (At one point she fleetingly reminds him of the murdered niece.) Sonja finally gets his message, that he himself was the ax-murderer; she recoils in horror, and then throws herself to her knees before him and cries out, "*Vy*, what have *vy* done to yourself!" (*chto vy, chto vy éto nad sobój sdélali*). Raskolnikov responds, "How strange you are!" (*stránnaja kakája ty*). It is at this critical juncture that Sonja at last switches her pronouns: "No, there is no one in the entire world more unhappy than *ty*" (*net, net tebjá neschástnee nikogó v tsélom tsvéte*, 439). She immediately repeats *ty* in several sentences while venting her own compassion, and pledging her willingness to follow him anywhere. Sonja's pronominal switch, just like her kneeling, exactly parallels the earlier scene, and is reminiscent of the eruption of *tu* at the most climactic moments in *Phèdre* and other plays of Racine.

After her rush of commiseration, however, Sonja thinks of the murder, of the fact that Raskolnikov has bludgeoned her friend, the niece of the usurer. She pulls away, and also switches back to *vy*: "Yes, but what are *vy*?" (*Da kak vy, vy takój*, 440). Raskolnikov then explains that he may have killed in order to steal, at which Sonja's love comes flooding back, with a switch to *ty*: "*Ty* were hungry. *Ty* ... in order to help your mother..." But when he denies this, Sonja, again horrified, reverts to *vy*: "How do *vy* give away your last money, but kill...?" At this point Raskolnikov is torn between his growing affinity and sympathy for Sonja, and his desperate need to be frank about his own cynicism and immorality. He becomes agonized, wonders out loud why he has come to confess at all, and exclaims that they are two very different (*róznnye*) people. Here Sonja, again seeming to sense his loneliness and despair, switches back to the pronoun of intimacy with "(*Ty*) speak, speak!" (442). This exhortation precipitates Raskolnikov into his theory of the Napoleonic hero: would Napoleon have murdered in defiance of the moral law? Sonja, confused and irritated, switches back again to the formal pronoun: "*Vy* had better speak to me plainly, without examples" (443). At this Raskolnikov launches into a third line of explanation: he might have murdered to help his mother and sister, but the genuine reason was that he wanted to dare (*osmélit'sja*). Sonja is yet more estranged and cries out, "*Vy* have left God, and he has struck you down" (*ot bóga vy otoshli, i vas bog porazil*);

then, "Vy be quiet! Don't vy dare, blasphemmer" (446), and "Do vy have the right to kill?" Raskolnikov, persisting in the exposition of his philosophy, states that he just wanted to try something out (*popróbovat'*), but soon realizes the hollowness of his position. He appeals to her for advice. True to her character, Sonja responds to him, and changes pronouns for the seventh and last time in only nine pages of dialogue, "(Ty) rise up..., (ty) bow down at the crossroads..., (ty) confess to all the world" (448). This unforgettable scene of the gradual union of two human beings ends with Raskolnikov's agreement to go to the police eventually, and of Sonja's gift of a cross to be worn around the neck (*natél'nyj krest*), which had been presented to her by the murdered niece. As when urging him to confess at the crossroads, Sonja's offer of the cross, a symbol of ritual siblinghood, links this novel of intellectuals and outcasts to some of the most enduring and profound dimensions of Russian peasant culture. Just as in *The Idiot* and *The Brothers Karamazov*, the theme of solidarity between brothers and ritual siblings underlies much of Dostoyevsky.

Toward the end of the book (e.g., 556-8), the two young people use only *ty* to one another. Thus, an elective affinity gradually guides them through many stages of pronominal usage with a frequency and rapidity of change that is at times difficult to follow. Starting fairly close, as destitute and demoralized individuals of good family, they have been drawn yet closer, as a guilt-ridden murderer and an unwilling prostitute, and finally as husband and wife about to share the rigors and deprivation of a protracted exile in Siberia. Their relation differs profoundly from that depicted in *Resurrection* between a status-conscious blue-blood and a practiced whore.

#### A SPECIAL KIND OF SWITCHING: THE EXPRESSIVE AND THE LATENT TY

The present study, while primarily concerned with the cognitive aspects of culture, also touches at many points on sentiment and feeling, both as they were regularly patterned and in their idiosyncratic manifestations. Some mood, whim or mental state could make the speaker play with or altogether ignore the usual rules, depending, of course, on his emotional makeup and social sensitivity. In other words, emotions of a certain sort generated pronominal usages quite different from those described above. Even the causation of this usage was different because norms and rules were less important, and because the function of the pronoun was often latent or expressive.

To begin with, the second person pronouns — notably when in rapid-fire alternation — could express purely idiosyncratic impulses or the peculiarities of a situation. The shift to *ty*, both more frequent and more charged with emotion, was often an unconscious slip or outburst that, because of its transitory or covert character, did not necessarily evoke or reflect a mutual restructuring of the relationship. For example, *ty* was simply part of many petrified phrases of surprise, fear or other strong emotion; thus, a country squire on fairly formal terms with a visiting gentleman, might burst out

momentarily with a grateful, "Ah, *ty* are my benefactor" (Gogol, *Dead Souls*, 89). A maid normally on asymmetrical terms with her master might cry out, on bumping into him in a 'dark corridor, "Ty, my lord, watch out!" The informal pronoun was part of many frozen expressions such as, "The devil take you (*tebjá*)!" Such exclamations often did little more than express the speaker's sentiments about a touchy or startling situation; but at another level of contrast, they at least implied a fleeting and superficial shift in his relation to his interlocutor.

In the second place, personal derangement was also signalled by the excessive use of *ty*. Many persons when drunk grew familiar with all and sundry, and in one striking scene from Lermontov (*Hero of Our Time*, 232) a drunken and berserk Cossack *ty*'s an officer while cutting him down with a saber. More permanent disorganization was often associated with a readiness to use *ty*; since such regression was always to *ty*, it may be regarded as the unmarked or generic form. An aristocratic woman, the daughter of a general, is gradually dying of tuberculosis in a squalid one-room apartment, surrounded by her starving children. Half-crazed and hysterical, she turns on the companion of her alcoholic husband, crying, "From the tavern! *Ty* were drinking with him! (*Ty*) get out!" (Dostoyevsky, *Crime and Punishment*, 32). Later, at the husband's funeral, both go back to their usual *vy*. But a few pages later her maternal instincts are exacerbated by the guilt she feels over her daughter's condition (421), and she lashes out with *ty* no less than six times on half a page, and many times more if one counts the verbal forms that imply the pronoun. When she is lacerating a vicious gentleman who has accused her daughter of theft, she repeats, "*Ty* look then! (*Ty*) look, (*ty*) look, now (*ty*) look!" This exclamatory pronominal usage symbolizes her abject despair. The extreme use of the *ty* of pronominal instability was part of hysteria; the onset of insanity was signalled by a neutralization or cancelling out, in Prague School terms, of the distinction that set off the two pronouns from each other. I accept as apparently sound the theory of Jakobson (1932: 83) that individuals in aphasia, extreme senility, and so forth generally revert in childhood to the unmarked form.

The informal pronoun could also serve as the one-sided expression of an extreme ideology which, from one point of view, was a lack of contact with 'reality'. One handsome and egregiously chaste young woman "was on *ty* terms with everybody" (Tolstoy, *Resurrection*, 416), as part of her revolutionary posture. An old wanderer and anarchist, who had been jailed once for "insanity", "believed only in himself" and used *ty* to all (437-8). Speaking more generally, the exclusive use of the informal pronoun often symbolized an outlook on man and society characteristic of the insane, the senile, hermits, and extreme revolutionaries, notably terrorists.

The 'second person singular' was also used by normal persons in normal states to express involuntary or transient feelings of contempt, hostility, and the like, often quite independent of the social class of the participants to the speech event. In one instance, a poor village priest consistently reciprocates *vy* with the niece of a powerful landlord until he is questioning her about life as an actress, when he says, "Sometimes

with a spitty snout — one ought to be forbidden to even look at him — but *ty* have to proffer your lips” (Saltykov-Shchedrin, *Golovlyov Family*). His switch of pronouns here was apparently unintentional, but the effect on his listener of the message, and of the pronoun itself, was devastating. In another case, a doorman, when turning away a gentleman, thinks to himself, “Just the same, if they chase *ty* away from the threshold, then *ty* must be some sort of rascal” (Gogol, *Dead Souls*, 153). In a third case, a young lackey, when fussed at about fixing some blinds, thinks to himself with reference to the princess, “But the devil take *ty* if I know what *ty* need” (Tolstoy, *Resurrection*, 100).

Such shifts in pronouns could explicitly mark a patterned shift toward contempt or vilification; the big trading houses had special agents known as “nephews” to make false bids at auctions, among other things. When a nephew fell into disfavor and was about to be booted downstairs, “the director would begin to interlard his speech ever more with the pronoun *ty*, and with polite epithets in the nature of ‘rascal, good-for-nothing’ ...” (Zlatovratsky, *Foundations*, 297). Contempt or dislike could always develop into hatred, and this would be appropriately signalled. In one case, a housemaid has been forced into concubinage, but directs her frustrated emotions into maternal love for an illegitimate child, until it is torn away and sent off. From then on she thinks only *ty* to her master and tormentor (Saltykov-Shchedrin, *Golovlyov Family*, 232), although she sticks to *vy* in face-to-face conversation. Personal hostility or even hatred, an important force in both peasant and gentry culture, could be expressed through the consistent although entirely mental and therefore unreciprocated use of *ty*.

On the other side, affection or love could also be projected through the covert and idiosyncratic thinking of *ty*. One aristocrat, when passing through the initial stages of a liaison with a seductive St. Petersburg lady, finds himself conversing with *ty* in “the language of the eyes”: “The eyes said, ‘Can *ty* love me?’ and the answer was, ‘Yes’” (Tolstoy, *Resurrection*, 300). Almost all Russians, including the gentry class, were potentially or mentally on *ty* terms with a larger number of persons than one could deduce from their overt linguistic behavior.

These intuitions, shared by all normal Russians, were made delightfully explicit by Pushkin in a verse entitled “Ty and Vy.” The last quatrain is:

Пред ней задумчиво стою;  
Свести очей с нее нет силы:  
И говорю ей: как вы милы!  
И мыслю: как тебя люблю!

Pensive I stand before her now  
And cannot tear my eyes away.  
“How dear *vy* are,” I say,  
And think, “How I do love *tebjá*.”

But, except among the peasants in their communes, such latent usage also mattered for the lower classes. In one instance, a proletarian mother in her forties says, "(Vy) farewell", to a young female worker, but, looking after her from the window, she thinks, "Comrade! ... ah, *ty* dear, may God give *tebé* an honorable (*chestnógo*) comrade for all your life" (Gorki, *Mother*, 153). The same proletarian mother-image regularly thinks *ty* but says *vy* to her son's sweetheart, an educated girl of vaguely middle-class background. She "felt a desire to say to her, 'My dear, indeed, I know that *ty* love him...' But she could not make up her mind to it" (*ne reshálas*). And then later, "She pressed the extended hand and thought, '*Ty*, my unhappy one.'" But soon thereafter she says aloud, "*Vy* speak truly" (196).

The foregoing data demonstrate some of the functions of the latent or mental *ty*, and this relates to a more general point. One of the fallacies of behavioristic descriptive linguistics and of behavioristic social psychology is that, by a sort of convention, the evidence is artificially limited to the overt, actually articulated forms. Such an approach, while usually valid for phonemics or many kinds of socio-metrics, is apt to lead to distortion in semantics, because so much of meaning is private and never made explicit during the act of speech. On the one hand, such inner life is often maximally expressive, although barely reciprocated unless the addressee picks up the cue of gesture, intonation, and so forth. On the other hand, some of the most trenchant communication involves the combination of one spoken pronoun with paralinguistic features of body movement and intonation that would normally accompany the covert unspoken pronoun; thus, an explicit *vy* combined with paralinguistic *ty* (warmness of tone, and so forth), could often signal a felicitous union of personal respect and an affection whose strength was not mitigated by overt restraint; an overt, spoken *vy* when conjoined with the modulations and gestures usual for certain *ty* bonds could often signal revilement and disgust in the most painful manner. Unfortunately, the evidence being considered here does not permit many inferences of this sort; but by the same token, the singular value of novels is that, unlike plays or psychological experiments, or most field work, they do provide evidence of what speakers would THINK in a variety of situations and status relationships.

#### POSTSCRIPT

Pronouns display unusual properties of emotional expressiveness, logical abstraction, and frequency in dialogue. Pronominal sets, like those of kinship terminology, are Janus-faced because they are linked into both the linguistic matrix of grammatical paradigms and the cultural matrix of social statuses and group categories. Among the many symbols of status in Russian, the second person singular pronouns were the most pervasive, frequent, and profound in their implications. Just two short words, operating in all speech events that involved two interlocutors, signalled the relative position of each pole in hundreds of dyadic relationships. Analysis of a representative

sample of pronominal usage leads to the inference of a relational system of positional slots in the status system, and of other culturally specific categories. For Russian the categories which have to be postulated in order to predict pronominal usage more or less correlate with those inferred on the basis of external socio-cultural evidence. The same, ego-oriented analysis of pronominal usage leads to the inference of discriminations or components that had to be controlled by the Russian in order to speak and understand. In the nineteenth century, at least, ten discriminations underlay the overwhelming mass of pronominal usage: topic of discourse, the context of the speech event, then age, generation, sex and kinship status, then dialect, group membership and relative jural and political authority, and finally, emotional solidarity. All these discriminations, in modified form, also underlay the equally important symbolism of the kinship terminology (Friedrich 1964). Such overlap of components in even these two semantic subsystems suggests that a continuing analysis of social structure would lead to an ever diminishing margin of discovery of discriminations, which are probably quite limited in number. Also, about six to a dozen such discriminations probably underlie most systems with two pronouns. The ten discriminations with their patterns of combination constitute the stable — though not static — system that accounts for nearly all the evidence and predicts with accuracy the usage in new texts.

The Russian usage was further characterized by a great deal of switching, by sudden changes of pronoun during a conversation or new encounter. Precisely because it depends on addition, loss or realignment of discriminations, switching gives invaluable evidence on relative or hierarchical order. But while many important relationships between discriminations can be inferred, the total network is less accessible. This is partly just because some evidence is lacking. Moreover, a decision as to the relative status of any two discriminations can only be made within realistic contexts that involve several others; the total number of such possible combinations is very high. In addition, graphs of the relations between discriminations would only be valid if made for precisely defined dyadic pairs in precisely defined contexts; the axes would have to specify many subdivisions of the ten discriminations used above.<sup>26</sup>

Switching and what I have called 'pronominal breakthrough' are interesting also because they are motivated by an interplay of the aforementioned cultural principles with what were essentially psychological forces of hostility, affection, ambivalence, mental derangement, and the like; the role of emotion was comparatively great in the Russian way of life. Insight into the psychology of pronominal usage may be had, not only from switching, but from the expressive or latent use of the informal pronoun. In many cases, documented by the realistic novel and experienced by any

<sup>26</sup> There is no reason why the analysis presented above could not be adapted to a digital computer, except that it would require far more steps, and would distort the network relationships that form an essential part of my theory. Such computational analysis, together with psychological and experimental approaches, would lead to many new questions. For example, which components show the greatest independence and interdependence? Is a more uniform specification of the components possible, and if so, how would it contribute to more accurate prediction? What is the relative power of the components?



normal Russian, the informal pronoun was encoded mentally, often embedded in phrases of love or hate, even while the formal pronoun was being articulated in a more or less different context. I take this to be but an accessible variety of the more general phenomenon whereby people think or feel one continuous message while enunciating a second string of overt forms. The two messages can range in content from virtual identity to total divergence. A linguistic theory which boldly exploits all available evidence and intuitions about such concomitant inner speech and other 'psychological' phenomena will not only have grappled with some of the most challenging experience, but will be capable of predicting future events more fully and realistically.<sup>27</sup>

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## DISCUSSION

M. Ivić: The opposition *ty/vy*, which reveals a peculiar social attitude, has (in Russian as well as in some other Slavic languages) a correlate in the opposition *on/oni*: the first of these two pronouns refers to the third person singular (masc.) and the second is the normal form of the third person plural used for the singular as a POLITE form. However, this kind of use of the pronoun *oni* is, in contradistinction to the similar use of *vy*, socially marked: it occurs only among non-educated people (*oni skazali* would, for instance, be said by a peasant girl speaking of her father).

As regards the *ty/vy* opposition, it should be noticed that, in contradistinction to the educated people, the non-educated peasant would use the *vy* form when addressing a person who belongs to a higher social rank, but the *ty* form if his fellow peasant is concerned, even at the occasion of their first meeting.

Finally, a quarrel can change the type of address: two educated people, being previously on *ty* terms, may switch to *vy* and vice versa.

MATHIOT: Friedrich's paper, with the additional cases presented by Mrs. Ivić, shows the complexity of the situation that arises when one deals with a grammatically marked opposition. I would like to suggest considering the English case, which would be an interesting parallel to the situation described by Friedrich. In English there would be terms of address which are not grammatically marked but lexically marked. This should lead to some interesting comparative knowledge about what one can infer from the grammar and what one can infer from the lexicon.

GUMPERZ: I noticed that in your examples, when people addressed each other by kinship terms, they tended to use *ty*. When they addressed each other by more formal names, they tended to use *vy*. Is there any kind of pattern to this, so that the use of kinship terms co-occurs with *ty* as a marker?

FRIEDRICH: I see your reasoning, but the co-occurrence is of a sort that requires some qualification. The peasants said *ty* to each other, with the exceptions noted, and used kinship terms in the usual sense, as well as forms like *mátushka*, *bátjushka*, *brat*, extended to certain categories in the society in what might be called pseudo-kinship usage. Among the aristocrats this pseudo-kinship usage often would be correlated with a switch to the informal *ty* pronoun. But the striking thing is how often the same pseudo-kinship terms are used together with the formal *vy* pronoun: a way of reaching out and figuratively touching a person while remaining pronominally formal. By calling the person *bátjushka* "dear little father", you circumvent the felt artificiality of the *vy*.

GUMPERZ: This brings me to just one other question. What you are studying is really what sophisticated literary men think about rural behavior. If you studied verbal rural behavior, would you expect to find a more fragmented pattern?

FRIEDRICH: I doubt it. You are right that I'm studying educated behavior, in the sense that the novelists and their audiences were all educated. On the other hand, I'm personally confident that novelists such as Tolstoy and Gorki understood the

classes which they wrote about, although they didn't necessarily belong to them. Tolstoy understood peasant usage very well. He lived with them and talked to them a great deal. And his choice of pronouns would have to be acceptable to his Russian readers, who of course knew the facts of usage.

FISCHER: You suggested that in German you don't get very much of this pronominal switching, although you do have the two forms, of course. In Japanese you do get some pronominal switching, but you have a sort of interesting additional phenomenon: this is the entire avoidance of the pronoun. You don't get a direct switch from one pronoun to another pronoun without undergoing a period of complete avoidance of any pronouns for a substantial period. Also, there are some individuals, including some husbands and wives, who never use pronominal forms or personal names to each other.

You suggested also that in Russian you get an increase in frequency of pronoun use in situations of great emotion. Perhaps in Japanese the avoidance of the pronoun is an attempt to avoid expression of emotion. The Japanese have a lot of emotion, they are very sentimental people and all that — but they do try to avoid the public, overt expression of it.

FRIEDRICH: I would say emphatically that consideration of certain units such as the pronouns, which are somehow keystones of many kinds of behavior and thinking, is a way of getting to much more general factors of ethos in the culture. I think that your comment as to the basic ethos underlying differences in Japanese and Russian usage in times of stress is intuitively quite sound.

DILLON: What I want to say may seem frivolous, but the speaker's concept of stratification of Russian society reminded me of a story about Irish society, where conditions are rather similar because we've never had an industrial revolution. Mrs. Yeats, the wife of the poet, once told me that, when she came into Ireland as an Englishwoman, what astonished her most was the extraordinary stratification of Irish society. And she told me the following amusing anecdote. When Yeats was living in his tower (some of you will know of that period), Mrs. Yeats heard that the Archdeacon of Gort had recently returned from the Far East and she thought it would be fun for Yeats to meet the Archdeacon. And so she invited him to lunch and the lunch was very successful. The following day she bicycled over to Lady Gregory's house at Coole and told her that she'd had the Archdeacon to lunch. And Lady Gregory said, "To lunch, my dear? Tea would have done!" For Lady Gregory there were three sorts of people in the old society of Galway: there were people whom you had to tea, and there were people you might sometimes invite to lunch; and then there were the old twelve families of Galway whom you occasionally had to dinner.

Friedrich's paper shows how much we miss, those of us who have to read Russian in translation, by not being able to pick up these sensitive things. One other example which is relevant to the problem is a lovely collection of letters between Sir Frederick Pollock and Chief Justice Holmes (Howe 1941). Pollock was an elderly aristocrat, and Holmes and he corresponded for years. And as they become more familiar, Holmes

tries several times to call Pollock "Fred" in his correspondence, but every time Pollock — although they were great friends — calls him Holmes again. Holmes even says in one of his letters, "Couldn't we drop it now?" But Pollock does not respond.

One other word that has to do with the use of the pronoun: the distinction between *vous* and *tu* has invaded the Gaelic dialect of Scotland on account of the French influence in Scotland. When I first went to study Scottish Gaelic, I couldn't understand it. The first morning, the woman addressed me as *šu*, which is the plural pronoun. I found that coming as I did from the University, having studied Gaelic as a foreign language, I was in another class; I was addressed as *šu*, and put in the parlor, and I couldn't mix in with the people at all. In Ireland there is no *šu*. If you go down to the Aran Islands to learn Irish, you're brought into the kitchen, you are called by your Christian name, and you're on equal terms with everybody. The very absence of this *šu* makes the whole social relationship easier. I never got on terms with the people in Scotland at all because they couldn't forget that while they were *tu* to each other, I was a *šu* and I had to be kept at a distance.

MCDavid: I would like to make a few observations about another disappeared and stratified society. Since no member of the in-group of that society, such as the Northwestern Nemesis Mr. Sledd (now the Texas Terror), is around to contradict me, I can at least approach it with some semblance of objectivity. I have been struck by a number of resemblances between the old Russian system, as outlined by Friedrich, and that of the Old South. First, I was reminded of one of Leonard Bloomfield's favorite stories, about his great-uncle Jaeger's mother, who was brought up in Galicia. As I understand it, in her community the quality spoke German, the townspeople spoke Polish, and the peasants spoke Ukrainian. Then Great-Uncle Jaeger's mother and family migrated to the United States, and settled in a small town in southern Alabama. There she made the natural cultural transference: the few plantation people she ran into, she tried to speak to in German; the townspeople she addressed in Polish, and the Negroes in Ukrainian.

This transfer is really not too far-fetched, though, when you consider some parallels in detail. In the South, there was an almost extravagant use of titles by Negroes in addressing whites — military, political, even ecclesiastical titles: Captain, Colonel, Judge, even Reverend or Bishop if a white patron or potential patron seemed to carry an aura of sanctity. Second, there was a use of kinship terms to closely associated persons of the inferior status group. It is generally known that *uncle* and *aunt* were used by properly educated whites, both young and not-so-young, to address elderly Negroes, especially long-term family retainers. So was *Mammy*, as in *Mammy Julia*, and its variant *Maum*, still common in Charleston. Even *Daddy* was so used. One of the frequent comments from my older low-Country South Carolina informants, of the upper-class white group, ran something like this: "We were shocked when we heard white people from the Up-Country addressing their fathers as *Daddy*, because in our childhood we had been taught that *Daddy* was a term to be used in addressing elderly Negroes."

Another feature of the Old South was the fact that, as in Ireland and some other places, people who could claim some remote connection by blood or marriage or even long-term family friendship could refer to each other as *Cousin* if they were of the same age-level, or the younger could refer to the older as *Uncle* and *Aunt*. This tradition is continued in a somewhat jocular vein by Freeman Twaddell and me; because we both got our start in linguistics under Paull Baum at Duke, we feel we are entitled to address each other as *Cuddn Raven* and *Cuddn Freeman*. Finally, a matter of social feedback. Throughout the South, *aunt* — in the normal Southern and south Midland pronunciation [ænt], with varying vocalic qualities and alternate tragerizations — has been associated by Negroes with a subservient rôle. Consequently, as Negroes acquire education and status, they tend to reject [ænt] for [ant]. And of course, so far as Southern whites are concerned, the fact that Negroes favor [ant] as a prestige pronunciation is sufficient to destroy any prestige that might normally be associated with the Eastern New England or Southern British pronunciations of “aunt” and similar words.

FRIEDRICH: You mentioned the different kinds of symbolism and the fact that there was an increase in the number of terms because they’re used to categorize people. Stankiewicz (1958) has written an article in which he points out that the Slavs used kinship terms for malevolent spirits. The devils were often called uncle or grandfather. And the Russian term for grandmother has the same root as butterfly, and the butterfly is associated with the soul of the dead flying away. His point was that this usage is associated with taboo, and when the association of the kinship term with the malevolent spirit became particularly strong, then taboo came in and they began using another term for the real relative. I think here that you have a similar process of proliferation of kinship terms because they are used for many functions outside that of kinship.

HAUGEN: I would have been much happier if you had related these ten components of yours to the system set up by Brown and Gilman. To me it would be more interesting if you did not have to set up a series of ten more or less arbitrarily chosen components. Why ten? The relationship seems to be basically one of solidarity — either there is or is not a barrier between the speakers. Then you ask yourself what kind of barrier — is it one of power or is it one of unfamiliarity? Then you can define what the situations of power or of familiarity are. Your ten items seem to me to fit in very nicely as a second rank of components.

FRIEDRICH: I’m sure that there is an arbitrary element in these ten components. However, I was trying to be as unarbitrary as possible, and my response would be that there happen to be ten not because it is an even number (or something of that sort), but because with these ten I can account for changes, for switching, and for regularities. I don’t see how I can collapse two of them without having to then set up two sub-categories for the collapsed one. On the other hand, as I said, Roger Brown’s model of power and solidarity is very valuable, but perhaps more at a comparative level, because if you say that usage in Russian was based on greater solidarity,

then you have to explain what you mean by solidarity to account for it. So you end up with the ten components anyway. This is a little bit like a phonemic problem in which you can have one phoneme with several allophones and several rules, or you can have two phonemes with fewer rules and allophones. Something like the latter may be much simpler for certain purposes, and that's why I like this ten-component system.

HAUGEN: You also refer to the Whorfian hypothesis, and therefore I venture to allude to it again. The pronominal systems of certain Indian languages have been used to illustrate one approach to the Whorfian hypothesis. The assumption that these are grammatical features, however, is open to some discussion. Is this relationship of *tu* and *vous*, or its equivalent in any other language, really a grammatical fact? Is it not also a lexical fact? In English we have abandoned it completely and yet we have completely parallel linguistic systems in the use of first names, last names, and so on, which we would all agree are lexical.

Finally, I have one little story, though it doesn't compare at all with the excellent one told by Myles Dillon. I was at a quite formal dinner in Oslo, a University dinner, and my partner at the table was a lady professor at the University, who was a member of the Labor Party. She asked me if I could tell her the political party of the gentleman on her right. She had to know before she could address him. I said I thought he was a Labor Party member. "Well, that's fine, because then I have to say *du* to him." The point was, of course, that revolutionaries all address each other as *du*. And in Norway, of course, the revolutionaries are well-established members of society.

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# SOCIO-CULTURAL AND LINGUISTIC FACTORS IN THE DEVELOPMENT OF WRITING SYSTEMS FOR PRELITERATE PEOPLES

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This paper presents a tentative analysis of some research findings that stem from a long-term project in which I am presently engaged. My goal here is simply to isolate some of the more important socio-cultural and linguistic variables that affect the creation and development of orthographies for unwritten languages. Through illustrations drawn from my own interviews and questionnaire data, I seek to show how these variables operate in specific situations.

My long-range goal is to formulate certain well-defined hypotheses — i.e. statements like, “If such and such conditions exist, certain other patterns can be expected.” These hypotheses, added to existing formulations (e.g., Pike 1947), could serve as principles for guiding linguists in their construction of orthographies for preliterate (or largely nonliterate) language groups.<sup>1</sup>

## HISTORICAL BACKGROUND

It has been my hypothesis that the relationships between writing and speech vary significantly with the nature of the socio-cultural order (Sjoberg 1964). Fundamentally I distinguish three types of society: preliterate, preindustrial civilized, and industrial. Preliterates have no writing. Preindustrial civilized orders are characterized by the use of writing, and by a panoply of social and cultural features generally associated with the presence of writing. But literacy is restricted to a small urban elite. Industrial societies, on the other hand, are characterized by mass literacy.

The reduction of preliterate languages to written form is as old as the history of civilization and the conquest of preliterates by civilized peoples. But until the recent past the functionaries concerned with this activity were usually representatives of the society's ruling group. Where they were interested at all in promoting literacy, they

<sup>1</sup> Under the term “preliterate peoples” I also include certain local cultural groupings, within national societies, which show the influence of writing systems deriving from earlier cultural contacts; these scripts are not now part of the national society's traditions, if they ever were. Examples are the Maranao in the Philippines, who include many persons who read the Arabic script; the Cham of South Vietnam, whose leaders can read the ancient Cham writing; or groups like the Miao, Eskimo, or Cree, for whom outsiders have invented special syllabic scripts. Such local societies lack an urban base and can be considered essentially nonliterate; from the point of view of persons attempting to bring them into the mainstream of the modern national culture, these communities generally present the problems of preliterates everywhere.



typically sought to impose the writing system of the elite upon the languages of the preliterate peoples in the society. The efficiency, economy, and consistency of this script and its appropriateness for the languages in question rarely were considerations. To cite an extreme instance, when Turkic peoples in Central Asia came under the dominance of Muslim culture, the Perso-Arabic script was as a matter of course accepted as the medium of writing for these languages. But this script, with its general neglect of short vowels, was a poor fit for languages in the Turkic group, some of which have nine or more vowel phonemes.<sup>2</sup> Of course, the propagation of mass literacy was not the intent. The purpose was to disseminate the dominant culture, to achieve and maintain communication between the rulers and the ruled, and thus to facilitate matters of government, particularly the collection of taxes or tribute and the suppression of rebellion.

The industrial type of society came into existence after the Industrial Revolution in Europe. Associated with the spread of industrialization has been the growth of democratic institutions. Both industrialization and democracy lead to (and require) concerted efforts to diffuse mass literacy — with emphasis upon education in the scientific method. The scientific orientation that underlies industrialization has also led to the development and spread of modern linguistics. All these factors, among others, have awakened an interest in the less prestigious language groups of the world.

A concomitant of industrialization, of the increased concern with mass literacy, and of the growing dependence upon the scientific method has been the rise of nationalism in many world areas. Nationalism has become a potent and a necessary force, especially in the new nations that have emerged from the former colonial holdings of Western industrializing societies — areas that included either preindustrial civilized orders (e.g., India) or traditional folk societies (the latter especially in Africa).

To promote modernization and industrialization, governmental leaders are finding it mandatory to absorb the surviving preliterates into the overall administrative and cultural system. Essentially, the goal is for all groups within modern nation-state systems to become literate. In many cases, particularly in Africa, the new nation-states include within their boundaries diverse language groups having little or no acquaintance with a written heritage. Uniting these divergent elements into some semblance of a national society wherein industrialization can proceed requires improved communication and, above all, widespread literacy.

Significantly, linguists have found that it is easier for a member of a preliterate group to achieve literacy in the dominant language of the nation-state if he first learns to read and write his mother tongue. In light of this, and given the previously mentioned world trends, the development of orthographies for the existing preliterate languages today takes on special meaning.

<sup>2</sup> Even in the case of Standard Uzbek, which has relatively few vowel phonemes, the current mode of writing (with the Cyrillic alphabet) provides a much more satisfactory representation of the language. Compare the Cyrillic with the Arabic writing of Uzbek words in the appendix to Akabirov et al. (1959, 729-839).

But devising efficient orthographies for unwritten languages is a task that requires the services of specialists — of linguists trained to analyze a language into its essential components and to select a minimum set of symbols that will mark all significant contrasts, yet satisfy the demands of practicality in the socio-cultural realm. Only in rare instances has an individual lacking knowledge of these special techniques been able to create a relatively simple and consistent orthography for his language.

Today, unlike the past, those persons who take on the task of developing orthographies for particular preliterate groups are usually versed in certain scientific procedures. Moreover, outside the Communist bloc the vast majority of orthographers are missionary-linguists. These persons not only utilize modern linguistic techniques but they have the dedication that is so necessary if one is to spend long periods in remote areas working with languages that may never have been analyzed. In some cases, several years of intensive study of a language in the field may be required before one can proceed to choosing an orthography for it.

#### BACKGROUND OF THE PRESENT PROJECT

Some comment on the pertinent literature should precede any discussion of this project. A few linguists, including those connected with missionary groups, have, out of their special contacts and experiences, developed some general principles in the realm of orthographies. Outstanding in this respect has been Kenneth Pike who, in his fundamental work on phonemics (1947), set forth a large number of phonemic principles in orthography-making.<sup>3</sup> His emphasis at the time was upon a one-to-one correspondence between phoneme and grapheme, where cultural conditions permit. Today he and other linguists recognize that cultural considerations actually are paramount and that a one-to-one correspondence is not often achievable or even desirable.

Pike's discussion of orthography in the above-mentioned work refers at various points to the impact of cultural forces upon the development of orthographies, as well as to areas of conflict between "phonemic goals" and "social goals". But it includes only a very few illustrations from specific language situations.

More recently, Berry (1958) has presented a valuable overview of the problems that beset the orthographer. Drawing mainly upon published sources but to some extent also on his experiences in Africa, Berry refers to a variety of impinging socio-cultural and linguistic forces.

However, no one has surveyed the diverse experiences of orthographers in a wide range of linguistic and cultural settings; nor has any one attempted to analyze the data

<sup>3</sup> A much-cited earlier source that advanced certain principles in the formation of alphabets in Africa is *Practical orthography of African languages*, revised (1930). Some quite recent guides for orthographers are Gudschinsky 1959, 1962. However, these are concerned mainly with the techniques of developing primers and with methods of teaching reading to nonliterate, not with the choice of orthographic symbols as such. Moreover, although the data are very valuable, they are, except for one brief reference to an African primer, drawn exclusively from the Americas.

on orthographies within a broad socio-linguistic framework. I believe it is possible to proceed much farther in isolating general patterns than the existing studies suggest.

Prompted by the lack of work in this area, and by my own concern with writing systems as a special field of study, I began some time ago to collect relevant information from a variety of published sources and through informal discussions with linguists with orthographic experience in areas such as Africa, India, and Southeast Asia.

Recently I initiated a survey using data obtained, via formal questionnaires, from persons in various parts of the world who are engaged in devising and testing orthographies for preliterate groups. I began this project after discussing orthographic problems with Iris Wares, a literacy specialist for the Mexico Branch of the Summer Institute of Linguistics. She herself has developed, or has collaborated with others in developing, orthographies or primers for a number of unrelated American Indian languages — from Aymara in Bolivia to Tarahumara in northern Mexico.

In September, 1963, with the kind assistance of Mrs. Wares, I was able to discuss orthographic problems with a number of linguists at the SIL headquarters in Mexico City and the Wycliffe Translation Center in Ixmiquilpan (in the State of Hidalgo). Not all of them were working on languages of Mexico; some were on leave from countries in Asia, where they had been concerned with matters of orthography.

On the basis of the information thus acquired, I designed a questionnaire; and in January, 1964, I revisited the SIL headquarters in Mexico City. A special meeting was arranged, attended by at least 20 persons who had been engaged in developing orthographies, and to whom I explained my project and my questionnaire. In the ensuing discussion, a number of valuable suggestions were offered for modifying and extending the questionnaire. Most of these persons subsequently filled out the forms, adding relevant comments where deemed necessary in light of the discussion at the meeting.

During the following months, I mailed the revised questionnaire to orthographers in various parts of the world. In addition I conducted interviews in depth with several orthographers, using the questionnaire as a guide. Most of the persons contacted are members of the Summer Institute of Linguistics — the largest group engaged in developing orthographies for preliterate peoples<sup>4</sup> — but a number belong to other missionary groups. I am also currently sending questionnaires to other persons with experience in devising orthographies, including university linguists and government officials in various countries.

By August, 1964, about 80 percent of the persons contacted had filled out the lengthy five-page questionnaire. As of that time, I had received over 120 completed forms from orthographers working with a variety of language groups in the following countries: Mexico, Guatemala, Honduras, Peru, Ecuador, Bolivia, Brazil, Colombia, Ghana, Australia, Australian New Guinea and Papua, West New Guinea (Indonesia), the Philippines, South Vietnam, Canada, and the United States (Indians of the Southwest, and Eskimos and Indians in Alaska).

<sup>4</sup> Nowadays the SIL (or Wycliffe Bible Translators) works only in those countries whose governments have requested its services.

The questions have usually been answered in fine detail, and a number of the respondents have taken pains to provide additional pertinent information. Many have sent me the primers and other initial reading materials that they have prepared in specific orthographies, so that I have well over 100 of these items. In many cases, the preface to the first primer (or primers) lists the phonemes of the language, along with the corresponding orthographic symbols.

The data gained from this research are especially valuable when it is realized that these orthographers are also actively engaged in testing the effectiveness of the systems they devise. Typically, from phonological and grammatical analysis of the language, they proceed to choosing a set of symbols to represent the language in writing, and then move on to the preparation of primers in these orthographies, designed to teach non-literates to read. Ideally, the same persons continue the work into the actual teaching, or applied, stage.

Because of their intense concern with one or a few languages or dialects, many of these workers are not in a position to discern the broader patterns or principles. It is here, by comparing the findings and experiences of orthographers in different linguistic and cultural settings, that I can perhaps make a contribution.

#### VARIABLES AFFECTING THE FORMATION OF ORTHOGRAPHIES

The variables that influence the choice of orthographic symbols or sets of symbols to be employed belong to two broad categories: the socio-cultural and the linguistic. Socio-cultural factors, as the term is used in this paper, refer to a group's system of values, ideas, and beliefs; its social structure (social class, political system, education, religion, and economics); and its technology. The second category — the linguistic — refers herein primarily to the phonological and grammatical structures of the languages in question. The matter of language structure, though clearly part of culture, is for analytical purposes viewed separately here.

However, as we shall observe, linguistic factors never operate in isolation. Nevertheless, they are fundamental to the shaping of any orthography — setting the first requirements. To create a scientifically based orthography one must FIRST have a clear grasp of the language's phonology, grammar, and vocabulary. And the modern linguist, unlike most earlier orthographers, consciously seeks to acquire this knowledge. At the same time, writing systems are definitely the product of socio-cultural factors which are often so numerous and varied and so intricately associated that it is quite difficult to separate them even for analytical purposes.

Before analyzing the influence of specific socio-cultural factors upon the creation of orthographies for preliterate peoples, we must consider the cultural traditions that have determined the linguist's own *Weltanschauung*, which in turn becomes an important variable affecting orthography-making.

*Broader Cultural Traditions and the Linguist's Scientific Heritage*

The particular tradition that is most meaningful for present purposes is our so-called Western culture, especially as it has taken form since the Industrial Revolution. Associated with industrialization, in fact basic to its development, has been the emergence of the scientific method. And, as we indicated, the advance of democracy and a growing concern with mass literacy have gone hand in hand with industrialization.

Modern orthographers, the particular applied linguists I am concerned with here, are products of a kind of learning that has come to dominate within scientifically based industrial orders, especially in Western Europe and the United States. (Some are also found today within certain sub-cultures of linguists in industrializing nations of Latin America, Asia, and Africa, who have been trained in or strongly influenced by modern science as developed in Europe and the United States.) Thus linguists are in general committed to the democratic system and the spread of mass literacy, as well as to the ideals of science, which not only calls for efficiency but judges results according to the pragmatic criterion of "what works best". This perspective has done much to shape current orthographic efforts.

Fundamental to modern linguistic science is the concept of the "phoneme". And phonemic analysis lies at the core of the linguist's efforts to devise practical orthographies for languages that may never have been set down in writing. The "phonemic ideal" of modern orthographers involves the representation of each phoneme of the language by a distinctive sign consistently employed. Though such a goal is not often achieved, it remains an overriding consideration.

Yet, with respect to the phonemic ideal, two fundamental questions arise in the realm of orthography-making: 1) Is a basically "phonemic" kind of writing — i.e. an alphabet — necessarily best for all languages? 2) Where an alphabetic system of writing is employed, what kinds of symbols should be involved?

As to the first question, some linguists have argued that certain languages might be represented most efficiently by a kind of syllabary. Indeed, during the nineteenth century and the beginning of the twentieth, missionaries untrained in modern structural linguistics devised syllabic scripts containing simple geometric signs for tribes such as the Cree or the Ojibway, various Eskimo groups, and the Miao and some other non-Chinese peoples in China (cf. Diringer, 1949, 1962). In the case of Eskimo, the syllabic writing is so well suited to the language that even today linguists, missionaries, and some government officials are reluctant to replace it by the Roman alphabet. The syllabic writing system, therefore, persists among the Eastern Eskimo even though various orthographies in the Roman alphabet are also in use within this group.<sup>5</sup>

<sup>5</sup> Syllabic characters are being employed today in a magazine for the Eastern Eskimo, *Inuktitut*, published by the Canadian Department of Northern Affairs. Moreover, since 1941 missionaries have been publishing a magazine for Eskimos in syllabic characters, *Inungnut Tamenut*. (See bibliography, Eskimo 1959a, 1959b.)

Although a syllabary may from many perspectives (including the ideal of efficiency) be best for the Eastern Eskimo, for example, it seems likely that the broader national considerations of Canadian society, wherein the Roman alphabet is paramount, will eventually lead to the exclusive use of alphabetic writing for Eskimo.

The second question above — namely, what particular symbols should be employed where an alphabet is used — is more central to our analysis. Within the Western tradition two major scripts, the Roman and the Cyrillic, have been utilized in the formation of orthographies for preliterate languages. The Cyrillic, which I shall not discuss here, has some advantages over the Roman: e.g., its majuscules in general closely resemble its minuscules. But for quite obvious socio-cultural reasons it is not employed outside countries under Soviet influence.

Looking at the problem more narrowly, we find that for a time, during the 1930's and 1940's, many linguists who developed orthographies for preliterate groups sought to write "unusual" phonemes with special symbols that had earlier been created for linguistic purposes. The most prominent of these symbol systems is the International Phonetic Alphabet.<sup>6</sup> Though its letters are based mainly upon Roman alphabetic signs, ideally they can be used to represent the sounds of any language.

One preliterate group which has been affected by the movement to write special sounds via traditional linguistic symbols (as well as to represent each phoneme by a single sign) is Tarascan in Mexico. Around 1940 several prominent linguists developed an orthography for Tarascan that included a number of special linguistic signs, and although a few of these have since been replaced by digraphs as used in Spanish or in the writing of Nahuatl, the Tarascan alphabet persists as a special case in Mexico.

In parts of Africa south of the Sahara, special linguistic symbols still appear in various of the orthographies, particularly those created by Britishers. However, the overall trend seems to be a retreat from these, with greater recourse to ordinary Roman letters and digraphs as used in the writing of English or French. Several reasons for this movement can be adduced. The leaders of the new nations, particularly in Sub-Saharan Africa, are generally intent upon emulating the orthographic patterns of the advanced industrial orders. They do not want their languages written with "strange" sets of signs. Moreover, there are pressures to employ symbols that conform to the needs of modern technology, which of course has largely been shaped in Western Europe and the United States. For example, it is easier to use typewriters or set linotype machines for ordinary Roman letters than for uncommon or specially created symbols.

Yet, this effort to avoid special letters leads to difficulties of its own. The fact that the Roman alphabet includes relatively few symbols — especially as used today in the writing of English — means that digraphs and trigraphs often must represent unit phonemes. And certainly the Roman alphabet is ill-equipped to handle languages with large numbers of phonemes. Furthermore, it has two minimum sets of signs: capitals

<sup>6</sup> In Africa the dominant system has been the so-called Africa Script (see bibliography, *African languages* 1930).

and lower-case forms. Orthographies in the Roman alphabet now being created by linguists for preliterate groups automatically include capital letters because this is the practice among users of the Roman in Western societies. This of course means a doubling of the number of signs to be printed and learned. Add to this the different shapes that many of the letters take in handwriting and in italics, and we find that the Roman includes a very large inventory of redundant signs.

But, ironically, while orthographers generally are reverting to the use of ordinary Roman letters for writing the special sounds of various preliterate languages, there are experiments in England and parts of the United States to teach reading by such systems as Pitman's Augmented Roman or Initial Teaching Alphabet, which includes a number of clearly "linguistic" symbols. Thus, whereas orthographers no longer place foremost the ideal of "one phoneme: one sign" — preferring instead to adjust to the dominant orthographic patterns of the enfolding society — some of their colleagues at home are inserting new letters into the Roman alphabet and combining several traditional ones in special ways in an effort to make the writing of English more phonemic.

#### *Present-Day Socio-Cultural Forces and Orthography-Making*

We can now say more about certain kinds of socio-cultural pressures, some of which have been alluded to above. Nationalism or "national needs" (including the demands of modern technology) is a major factor that frequently overrides linguistic considerations in the creation of orthographies. Consider a rather extreme instance. Utilizing the Roman alphabet, linguists recently developed an orthography for Tlingit in Alaska. However, the central dialect of Tlingit (the basis of the orthography) reportedly has 53 phonemes, 51 of which are segmentals. It is obvious that, given the small number of letters in the Roman alphabet, especially as used for the writing of English, a rather awkward writing system has resulted.<sup>7</sup> The problem is compounded by the pressures to follow the rather chaotic orthographic patterns of English wherever possible — in order to conform to the demands of the broader society and indeed of Tlingit speakers themselves.

More specific effects of nationalism can be observed from patterns in Mexico and other parts of Latin America. American linguists working with certain Mexican Indian languages have written the phoneme /ŋ/ as *ng* to avoid the need for special symbols and the resultant difficulty of typewriting and added costs of printing — only to discover that some government officials have interpreted their actions as an attempt to "make the language look like English". The cluster *ng*, of course, is infrequent in Spanish and is absent word-finally.

Not only does nationalism involve the rejection of "external" influences, but it induces glorification of the national language, including the latter's orthography. The effects of this in Latin American countries are much in evidence. For example, because

<sup>7</sup> See bibliography, Tlingit 1962, n.d. A very informative discussion of this problem is Naish 1962.

certain sounds in Spanish are orthographically marked, it is felt that similar sounds in the indigenous languages must be written also — whether they are phonemic or not. In Bolivia, national pressures have led to the writing of the Aymara allophones of /u/ and /i/, [o] and [e] respectively, because *o* and *e* are phonemes in Spanish. Somewhat the same situation prevails in the case of Chol in Mexico, as well as in many other Indian languages of Latin America.

Analogous patterns can be found in other cultural settings — e.g. in South Vietnam. In Pacóh the allophones of /w/, [v], [u], and [o], are all written because such sounds are represented in the national language's orthography. For the same reason, digraphs are used in Chrau to mark unit phonemes: *ch* for /č/, *nh* for /ñ/, and *ng* for /ŋ/. And because Vietnamese writes /k/ as *k* before *i* or *e*, but as *c* before other vowels, Pacóh and Chrau and other languages in Vietnam represent /k/ by two separate symbols.

On the other hand, those newly emergent nations that lack a strong unifying cultural heritage, including literary traditions that predate the advent of the Europeans, are, at least for the present, still looking to the former colonial powers as symbols of prestige. In Africa the leaders of the new nations react against attempts by linguists to introduce special signs so as to write a language in the simplest, most consistent manner — for, as indicated above, the native speaker does not want his language to “look different from” English (in the former British colonies) or French (in the former French colonies). For example, in parts of Africa where linguists have sought to write the phoneme /ŋ/ via the special symbol *ŋ*, opposition has been strong. Contrast this with the situation in, for example, Mexico, a country which now has a firm national image and a literary tradition.

Although national considerations loom large, orthographers must also accommodate to certain local and regional cultural traditions: these not infrequently are in conflict with the broader societal ones.

First, there are some local or regional cultures that have a history of resistance to absorption into the national society. In Mexico the Tarascans and the Isthmus Zapotecs are two such groups. In a sense, orthographers in recent decades have, at least in the short run, reinforced some of these independent leanings — although their ultimate goal is integration of these peoples into the broader society. In the case of the Tarascans, this group has an alphabet containing certain special symbols not used elsewhere in Latin America, and considerable quantities of reading matter have been printed by Tarascans in their alphabet as they have come to be transformed into a near-literate group.<sup>8</sup>

As for the Isthmus Zapotecs, this group has not only sought to maintain a degree of autonomy, but a number of highly educated Zapotecs have, over the past few decades, worked out their own personal orthographies for the language and have published poetry, essays, and the like in these media. However, in 1956 these cultural leaders met with two groups of linguists to work out a compromise orthography for

<sup>8</sup> Publications include a periodical, *Mitakwa*, which carries some articles in Tarascan and some in Spanish (cf. Barrera Vásquez 1953, 85).



Isthmus Zapotec. The resulting system includes a somewhat unique but not at all unscientific mode of representing certain phonemes. /ʒ/ is written as *x*, /ʃ/ appears as *xh* (the *h* indicates devoicing of the /ʒ/), and *dx* stands for /j/ (i.e. for [d] + [ʒ]).<sup>9</sup>

Second, there are certain broad regional languages that help to integrate diverse linguistic and cultural groupings within a political unit or even across national boundaries. In New Guinea (an Australian trust territory) it is Pidgin English that has permitted a degree of communication and cohesion among various local groups. Here the linguist, when he develops a writing system for a given language group, must take cognizance of the orthographies for New Guinea Pidgin. But since the sounds of the latter are those of the New Guinea languages, which in turn display quite simple phonemic systems, this poses few problems.

In the Philippines a unifying regional language, Visayan, competes in certain areas with the national language, Tagalog. Significantly, Visayan, spoken in the central islands, has about twice as many speakers as Tagalog, which is concentrated in the north, in and around Manila. Moreover, English enjoys a semi-official status and is the language of the highly educated within Filipino society. As a result, linguists who are developing orthographies for various local languages have had to adjust to a diversity of socio-cultural factors. For instance, those working with Dibabawon-Manobo in the central islands have had to take into account not only the orthographic patterns of Tagalog but also those of Visayan and English. Literacy in Visayan, the trade language, has been increasing among speakers of Dibabawon, with the result that more and more persons want their language's orthography modeled after that for Visayan. Moreover, the pressures to introduce English orthographic patterns complicate the picture considerably.

A third kind of regional or local factor affecting orthographies is the survival of earlier writing systems among peoples who, though functionally preliterate today, were at one time influenced by civilizations that have since receded. The writing systems of the latter still carry a high degree of prestige and may even be used by a few persons.

Linguists working among the Cham people of South Vietnam (cf. Fishel 1961, 99) are faced with the problem of the considerable prestige that still attaches to an earlier mode of writing in the area. This is the ancient Cham script, which finds its origin in earlier scripts of India. It is therefore a semi-syllabic, semi-alphabetic system wherein the alphabetic signs are grouped into roughly "syllabic" units. The intricacies of the script lead to difficulties in reading — many of the characters can easily be confused by the beginning reader — and the great number of "syllabic" signs adds considerably to the cost of printing. Yet, so great is the prestige of the older script among the leaders of the Cham that orthographers have had to prepare initial Cham reading materials in this traditional medium as well as in the scientific orthography (based upon the Roman alphabet) that they have devised. This has been necessary in order to en-

<sup>9</sup> See bibliography, Zapotec 1956, as well as issues of the newspaper *Progreso, Tribuna del Pueblo Istmeño* (México, D.F.), including the article by Velma Pickett (1963).

courage people to learn to read. Moreover, each page of the primers includes a translation into Vietnamese (written today in a modified form of the Roman).<sup>10</sup>

Another area where the survival of a traditional script has markedly affected the development of orthographies is on Mindanao in the Philippines. Here among the Maranao the Arabic writing, stemming from earlier contacts with Islamic civilizations, evidences continued prestige — so much so that it has slowed linguists' efforts in this area to spread literacy via the Roman alphabet. It is significant that SIL members working among these people today are writing their primers for Maranao in both the Arabic and the Roman scripts.

Fourth, orthographers often must take into account the values and beliefs — including certain aesthetic considerations — of the local group in question. For example, those working among the Pame and the Cuicateco in Mexico have asked individuals for their preferences as to certain symbols. Also, native speaker reaction may determine whether one writes given sounds morphophonemically or phonemically. Such has been the case among the Athapaskan Slave in Canada and the Chuj in Guatemala.

Problems also arise where members of the local culture interpret certain symbols in the proposed orthography for their language in a special way. For example, linguists schooled in the European tradition will, when they use accent marks, indicate high or rising tone by an acute accent, low or falling tone by a grave accent. But some Indian groups in Mexico connect rising tone with the grave accent: i.e. they visualize the latter as moving from lower right to upper left, rather than the reverse. Only when he makes this discovery may the orthographer realize just how arbitrary and culture-bound his own symbols for "high" and "low" really are. In other instances the orthographer may be aware of the native speaker's definition of specific signs yet find it well-nigh impossible to accommodate to these cultural factors. For example, some Indian groups in Mexico conceive of high pitch as "thin" and low pitch as "thick", where certain others define these in reverse fashion. The linguist, in keeping with his democratic ideals, may seriously consider writing pitch with accent marks of differing thickness, but the technological demands of the broader society — existing typewriters, printing presses, and the like — rule this out.

Actually, the symbolization of particular sounds is mostly a product of the interaction between the linguist's own cultural heritage and the socio-cultural traditions of the particular national society and local preliterate groups with which he is working. The complex interrelationships of these socio-cultural factors (implicit in the foregoing discussion) require considerable study. Frequently the linguist must arrive at a compromise among the aforementioned variables, with the result that the final solution may be less than satisfactory.

At times the linguist may mark certain sounds mainly for his own benefit — thereby

<sup>10</sup> See bibliography, Cham 1962. Incidentally, women among the Cham do accept materials written solely in the modern Romanized orthography, since they have had little contact with the traditional Cham writing. Literacy workers therefore make up two sets of primers and other initial reading materials: one for women and one for men.

compromising with the scientific ideal of economy. For example, if he is a speaker of an Indo-European language — English or French or Spanish, particularly — he will experience difficulty in handling long words in certain Mexican Indian languages. He may, then, resort to hyphens to break up these sequences so that he can better deal with them when teaching persons to read. Similarly, even in languages of the Zapotecan group, where tone does not carry a heavy functional load, the linguist may need to mark tone in the primers for his own understanding. In the above instances the Indians are reported to have observed: “You need them; we don’t.”

A further factor that influences orthographies are the linguist’s decisions as to which dialect of the language is to be the basis of the writing system. Ideally, the dialect chosen is that most representative of the group as a whole. But in practice it may be the one that is most accessible to linguistic analysis.

Where dialectal differences in a language are minor, the linguist often seeks to devise an orthography that will be usable for all or most of the local variants. Moreover, in the realm of grammar and vocabulary, in general the fuller forms of words and morphemes, as displayed in certain of the dialects, will be employed in the reading materials created in the chosen orthography. In Tarascan the degree of normalization is considerable. Here the linguists have also experimented with combinations of roots and suffixes in the language in order to devise ‘new’ words to express concepts unfamiliar to Tarascan speakers.<sup>11</sup>

### *Conflicts Between Socio-Cultural and Linguistic Factors*

Here space permits only a few case studies illustrating certain areas of conflict between the requirements of language structure and socio-cultural factors, particularly the need to follow the official orthography of the broader national society. (It is realized, of course, that the linguist’s desire to make his orthographies accord with the structure of the language so far as possible is itself a cultural factor — a product of his own scientific training.)

In Latin America dilemmas arise where an Indian language has both glottal stops and tones that must be marked for the sake of clarity. Inasmuch as the linguist’s sign for glottal stop, /ʔ/, is easily confused with the question mark by Indian groups who have some familiarity with the writing system of the national language, use of the apostrophe for glottal stop is generally preferable. However, recourse to the apostrophe may make the symbolization of tone difficult. If the linguist uses numbers to write tone he finds that the sign for “pitch one” tends to be confused with the apostrophe — not just because of somewhat similar shape but also because of similar position: both require a full space and appear half-way above the line. He may, then, have to mark tone via accent marks above the vowels. But what if the vowels are carrying other

<sup>11</sup> Information from interviews with Mr. and Mrs. Maxwell Lathrop of the Summer Institute of Linguistics, Mexico Branch, and the Tarascan Project, Cherán, Michoacán.

diacritics such as stress markers? Whatever choice is made will have its drawbacks — again, because the orthographer generally confines himself to the symbols of the national language, and adheres moreover to the traditional position of diacritics above the line rather than below or on the line to one side. If the linguist were permitted freer use of IPA symbols he could arrive at a more satisfactory solution, at least from a scientific standpoint.

In Mexico, as a result of the glorification of the Aztec heritage, an earlier use of the Roman symbol *x* for the /š/ of Nahuatl is maintained today. It is now the policy to write /š/ as *x* in other Indian languages as well. Linguists in most instances adhere to this pattern when creating orthographies. But what about a language like Seri which has, besides /š/ and /x/, also /x̣/ (a postvelar fricative)? Here an attempt to follow the national orthographic usage by writing /š/, and not /x/ or /x̣/, as *x* would constitute too extreme a divergence from linguistic tradition — a situation aggravated by the limitations of the Roman alphabet. It was therefore decided to use *x* for /x̣/, write /x/ as *j*, in the Spanish manner, and employ the symbol *z* (not otherwise needed) for /š/. *s* had to be reserved for /s/.

In other instances, the Spanish orthographic patterns are followed even where they contravene the linguist's own norms. For example, throughout Spanish America /k/ is usually written as *c* before *a*, *o*, *u* or a consonant and as *qu* before *e* or *i*. But some Indian languages also have /q/, which must then be symbolized as *k*. (Languages such as Aymara in Bolivia even have aspirated and glottalized counterparts of both /k/ and /q/.) In effect the orthographer finds himself having to write the postvelar stops with his traditional linguistic velar signs and the velar stops with his postvelar signs.

Further complications arise in Mayan languages such as Tzotzil and Chol. Besides a glottalized velar, these languages also contain the sequence /u/ + /ʔ/, written *u'*. Here the orthographers would have written /k'/ before *e* or *i* as *qu'* were it not for the fact that Indians learning to read their language might confuse the *u'* of *qu'* with the sequence *u'*. The linguists therefore decided to write the apostrophe *within* the digraph — thus *q'u*, a pattern that deviates from traditional modes of representation. For like reasons the apostrophe is written inside the digraph in Jicaque (Honduras), but here the sign for /k'/ is *c'u* before *e* or *i* rather than *q'u*.

The phoneme /w/ in Indian languages very frequently poses problems, for one must utilize the Spanish orthography wherein /w/ is written *hu*. In a language with the sequence /wu/ or, worse, /wuu/, writing these as *huu* and *huuu* would be rather cumbersome. Moreover, in languages like Jicaque where /w/ is often found word-finally, writing it as *hu* would mean that a word ending in a consonant would be written as if ending in a vowel. In Jicaque, therefore, /w/ appears as *w*.

A related problem concerns the writing of velarized stops such as /kʷ/ or /bʷ/. In Mexico, national and local cultural forces call for writing these as *ku* and *bu*, respectively. But Sierra de Juárez Zapotec has, besides a two-phoneme sequence /kʷa/, also a three-phoneme one, /kua/. Both are written *cua* in keeping with the Spanish

pattern. To resolve the obvious ambiguity the orthographers decided to omit tone marks in the first sequence but to write them elsewhere: thus *cua* vs. *cúa* or *cuá*. This, of course, means that some segmental phonemes are distinguished via suprasegmental markings, which contravenes the scientific ideal of consistency. In Yaqui the solution to the problem has simply been to write /bʷ/ as *bw*, despite national cultural influences to the contrary. The two-phoneme sequence *bwi* /bʷi/ thus is distinguished from the three-phoneme ones *buí* and *búi*.

These illustrations, drawn from the Latin American cultural scene, point up the special significance of language structure in situations where the linguist works within the constraints of a particular orthographic tradition that offers few symbols and, in addition, relatively little leeway in the utilization of these symbols, yet must create orthographies for a variety of divergent languages.

### CONCLUSIONS

My main purpose has been to isolate the kinds of variables that affect the development of orthographies for unwritten languages, and in the process to point up some of the implications of this neglected problem area for the field of socio-linguistics.

It is apparent that, in the realm of orthography-making, narrowly linguistic factors are much less important than socio-cultural ones. These include a) various facets of the linguist's own heritage, b) the national culture (including its particular level of technology), and c) regional and local traditions. I have attempted, via illustrations, to indicate how these differing factors are taken into account by orthographers when they develop writing systems for preliterate peoples. One firm conclusion emerges from the data: any scientifically based, yet socially acceptable, orthography for a given language represents a compromise among a variety of socio-cultural variables. I hope in the future to make these interrelationships more explicit.

### DISCUSSION

BRIGHT: I've thought that one of the more interesting publications in this field of providing orthographies for pre-literate peoples was Paul Garvin's article on the orthography designed in Ponape (1954). I'm wondering if Dr. Garvin would care to add anything to this discussion from his experience.

GARVIN: Well, to my mind there are two questions involved that are worth discussing. One is: what are the socio-cultural factors? It seemed to me that there were three kinds of factors that I ran across. One was pressure groups; the other was the existence of earlier traditions, and the third was just the general socio-cultural situation. A second problem which came up was the different ways in which Western alphabets can be modified, in particular the Latin alphabet, and the choices had to do

with two things. One was how to represent phonemes that do not have their equivalent in the Latin orthography; the second was features, such as the prosodic features of vowels, which also lack orthographic equivalents. Then there is the problem of the different European traditions. In the Mexican case there is only the Spanish tradition to consider, but in many parts of the world one can think of different kinds of European orthographic tradition, and one might find a population that doesn't mind being introduced to some other tradition than the one that's customary. So I thought that it might be worth considering how those orthographic systems can be classified that are in use in Europe: for instance, what kind of diacritics and digraphs are in use, and for what. In Ponapean I was particularly concerned with representing vowel length. And there were two solutions at that time which I thought were pertinent: one that I called the Finnish solution, that is, representing length by doubling the vowel symbol, and the other the German solution which is to write an *h* after it. In both cases the choice was motivated by the desire to introduce diacritics. The Ponapeans didn't want to have anything that wasn't on an American typewriter.

FISCHER: I should like to make a point about the Ponapean system. The first orthography was devised for Ponapean by an American missionary, who of course used English orthography as a model. Well, he assigned the letter *j* to a particular phoneme; but by the time the orthographic reform that Dr. Garvin worked on came about, this phoneme had shifted in value so that it was closer to English [s]. So you had American pressure for changing this symbol which had originally been assigned by an American in the first place. The *j* was in fact replaced by *s* in this reform.

DILLON: May I ask what the speaker's feeling is about the situation in Mexico? My feeling is that the authorities are right, so far as possible, in any attempt at a script for the Mexican-Indian languages, to take account of the Spanish writing system. I wonder whether linguists may not be fanatical in their desire to use the IPA, to try to get a one-to-one correspondence.

SJOBERG: A couple of decades ago the linguists were rather firm about using IPA symbols, but since then they have adjusted more and more to using the Spanish method wherever possible. However, Tarascan still goes on with its own IPA symbols. It is a very large and important group.

FRIEDRICH: Tarascan is a special case that has special causes. One is that this was the bailiwick of Morris Swadesh, and he spoke very good Tarascan while running the Paracho Institute there for two years. In addition, the Tarascans are in the middle of Cárdenas' political bailiwick, and he of course has enormous power and stimulated the Tarascan program much more than most other Mexican-Indian programs.

DILLON: Very few languages that I know, except Welsh, have an exact correspondence between spelling and pronunciation. I think that the linguist might get tangled up by his desire to record language phonetically. One should be rather empirical. The script, for teaching people to read, should be, taking everything into account, the most workable, and that may not be a phonetic script. The worst one of all is English, and somehow we manage to learn it.

BRIGHT: Irish is much worse...

SJOBERG: In the case of Chinese great difficulties could arise for meaning if you had a one-to-one correspondence between sound and symbol, because you have so many homophonous words that are distinguished by their appearance in the ideographs.

FERGUSON: Jack Berry (1958) has presented a very careful analysis of linguistic and non-linguistic factors in the creation of orthographies. I wanted to ask Mrs. Sjoberg whether, either in her questionnaires or personal investigation, she has come across any factors that were not hinted at in Berry's paper, or any modifications of the material he covered?

SJOBERG: Yes, I have, though I shaped the questionnaires to some extent in terms of certain points that Berry made. But most of Berry's examples were drawn from Africa, and the situation there is simpler. The main problem is: should you use linguistic symbols for special sounds or, in the former British areas, English orthography, and in the French areas, French orthography? Moreover, except in North Africa where you have to contend with the Arabic script, there really are no local traditional scripts to influence your decisions, as would be the case in much of Asia with its ancient writing systems. Even in Mexico, there is a very recent 'traditional' orthography — for Nahuatl — which clearly influences the orthographies of various Indian languages today. You don't have that in Africa; it's a much simpler situation. The problem there seems to be a matter of how to write special kinds of sounds, e.g. varieties of clicks, and not so much how to contend with older writing traditions in the area.

GUMPERZ: To some extent the competing missionary orthographies in Africa fill the function of the traditional orthographies in Asia.

SJOBERG: Yes, and the problem isn't made any easier by the fact that Africans themselves in the former French areas want their languages to 'look like' French, and those in formerly British areas want them to 'look like' English. They don't want their languages to seem very 'different' or 'strange'. They don't want new and special symbols for their often very special sounds. And that has been a real problem — getting native speakers to accept a special sign for a single phoneme rather than a series of Roman symbols — digraphs, trigraphs, and so on.

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# THE SOCIAL AND CULTURAL STATUS OF GUARANÍ IN PARAGUAY\*

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## 1. INTRODUCTION

1.1. This paper is not intended to describe the facts about the use of the Guaraní language in Paraguay, but rather the attitude of Paraguayans toward those facts. It will not be our concern to define or describe the language itself, except in instances which show misunderstanding of basic linguistic facts by speakers. Our aim will be to show that there is no correspondence between the Paraguayans' pride in their national language and their practical knowledge of what a national language should be like and how it should be dealt with, and that there is no proportion between their conviction that Guaraní is a suitable vehicle of culture and the amount of their conscious effort to develop its suitability. In order to show this, we will try to describe the importance of Guaraní in Paraguay (2), then we will discuss the social (both symptomatic and subjective) aspects of the language (3) and its cultural and educative aspects (4). Thereafter we will draw our conclusions (5).

1.2. The duality of attitude just referred to has scarcely been noticed by non-Paraguayan scholars, mostly because there is a surprising uniformity of attitude among Paraguayans. Since Guaraní is the national language, it is the main distinctive feature of the nation itself. It is therefore a most valuable asset for a nation which, from the very beginning of its history, has been ceaselessly opposed and very often attacked and laid waste by neighboring countries. This has been going on from the days of the Spanish domination and the expulsion of the Jesuit fathers, through the wars of independence, the Portuguese invasion, the "Guaranitic wars", the terrible and devastating war of 1865-70, and recently the war against Bolivia. The Spaniards who destroyed the Jesuitic Republic of the 16th and 17th centuries (in which Guaraní was the common language), like the inhabitants of Paraguay's neighbors, were Spanish-speaking. This gives the Guaraní language a very neatly defined national character, as opposed to Spanish (and Portuguese). Here we are facing the same kind of situation which made the Hebraic religion a powerful resource for the preservation of national or racial

\* Professor Rona was not able to be present at the Conference in Los Angeles, but submitted his paper by mail. It was introduced for discussion by Madeleine Mahiot. A transcript of the discussion was subsequently sent to Professor Rona, who returned the final comments which are printed below.

unity, or that which made the Romance languages emerge triumphantly from the universality of the medieval world. The attitude of average Paraguayan speakers towards Guaraní is basically the same as the attitude of Romance speakers of the 15th or 16th centuries towards their respective national languages. It is not an attitude of objective knowledge, but one of pride, of vindication or of defense. This circumstance is not always understood by foreign scholars who deal with Paraguayan informants. Facts regarding Paraguayan attitudes are thus permanently masked. This accounts for the fact that most of the papers that have been published merely repeat the assertions which one finds in the native literature. This has been clearly realized and stated by Marcos A. Morínigo, one of the foremost Paraguayan scholars (1959, 240 ff.).

1.3. While the Guaraní language itself has been thoroughly studied by some brilliant foreign scholars (e.g. Antonio Tovar, Bernard Pottier, and Aryon Dall'Igna Rodrigues) as well as by some Paraguayans (e.g. Guillermo Tell Bertoni, Antonio Guasch, S. J., and León Cadogan), its social and cultural status is very inadequately known. As stated above, almost all publications on this subject have been written by Paraguayans, who obviously cannot be wholly objective, since they are part — in fact a very active part — of the national process which made of the Guaraní language a banner of national recovery during the last few decades. We must therefore disregard their writings as a dependable source of information on our present subject, unless we take them as the subject itself, i.e. as plain evidence of the Paraguayans' attitude toward their language. This attitude can best be compared with that of the countless *elogios* written by Spaniards from the 15th to the 17th century about the Spanish language (cf. Bleiberg, 1951). Thus, for example, Jover Peralta, instead of describing Guaraní, states: "Es un lenguaje a la par rico y sabio. No hay idea, por sutil o abstracta que fuera, que no pueda expresarse en guaraní". Again: "Es uno de los idiomas más sabios y hermosos, más poéticos y filosóficos, y, a la vez, uno de los más antiguos del mundo, el más antiguo que se conoce" (1950, XV). We will see (4.1) that these statements, especially the first, are by no means true. Another Paraguayan scholar wrote: "Es que el guaraní, por la riqueza de su léxico, por la perfección de su estructura gramatical, por la superabundancia de palabras que posee para la expresión de conceptos abstractos y por la belleza y diversidad de sus formas de dicción, tiene la alcurnia de los lenguajes de los países de elevada cultura" (Bertoni, 1950, 87). We could go on quoting statements similar to the preceding, which are more frequent than true descriptions of the language or of some of its features. Foreign scholars have seldom focused their attention on the social or cultural aspects of Guaraní. When they do so, they seldom take into account the sound criticism and acute observations of Morínigo (1931). They prefer rather to pay attention to those Paraguayan writers or informants who, instead of telling us what Guaraní actually is like, state or describe how they would like it to be. Thus the well-known paper by Fogelquist (1950) repeats some of the commonplaces one may hear every day in Paraguay, e.g. the explanation of the native name of the language, *avañe'ẽ*, as "the tongue of the common man", and of the term for the Spanish language, *caraiñe'ẽ*, as

"the tongue of the masters", which seems in fact to be a fairy tale. Note also Fogelquist's comments on the "richness" of Guaraní in vowel sounds — true if compared with Spanish, but false if compared, for example, with the neighboring Portuguese, and incomprehensible when coming from a speaker of English — or his description of the central unrounded vowel *i* as "a French *u* gutturalized", which is simply a repetition of the inaccurate terms which Paraguayans use to describe it. A somewhat better discussion can be found in Einar Haugen's book on bilingualism (1956). The best approach we know of is the paper by Garvin and Mathiot (1960), but even these authors have been misled by some statements of their Paraguayan informants, as when they greatly overestimate the role of the Academia de la Lengua y Cultura Guaraní, of which even the name was improperly transmitted to them by their informants; or when they repeat an inaccurate explanation for the fact that Paraguayan Spanish is studded with Guaraní loans (cf. 2.4 in the present paper). These three papers by foreign scholars, and the two works of Morínigo that have been cited, are nearly all that has been written on the social and cultural status of Guaraní.

1.4. Our own concern with this problem originated when we visited the Paraguayan town of Encarnación, back in 1958, and found out that a good many people there did not speak Spanish at all. We noticed further that there was a somewhat contemptuous attitude toward Spanish among monolingual Guaraní speakers there. The common response of illiterate Guaraní speakers, when they were addressed in Spanish, was an attitude of mockery. This attitude was in open contradiction with what we had read about bilingualism in Paraguay. Later on, when we began to learn the Guaraní language, we had to face another characteristic manifestation of this situation: we found that there are numerous grammars and manuals of the language, but they are not in accord with each other as to grammatical rules, nor even as to orthography. All this stimulated us to pay more attention to the non-linguistic (i.e. social and cultural) aspects of Guaraní. Since 1961, we have been conducting research in this field, through (1) careful analysis of the responses of both monolingual and bilingual Guaraní speakers whom we have met either in Paraguay or in Uruguay; (2) a statistical analysis of Paraguayan bilingualism, conducted jointly with Guillermo Tell Bertoni, the late Chairman of the Academia de la Lengua y Cultura Guaraní (the results of which will be discussed in 2.3); and (3) a questionnaire that we submitted to twenty Paraguayans with a background of higher education. This questionnaire had been carefully prepared, so that the answers could be checked against each other and against facts known from other sources, and so that any difference between those facts and the answers to the questionnaires, or between divergent answers, could be evaluated as evidence of the speakers' knowledge or attitude, which was the kind of information we were looking for. Most of these questionnaires were never answered; in some cases there was no open refusal, only a lack of response. Some other questionnaires were returned with a polite statement that the addressees were not prepared to fill them out, because they had the feeling that they did not know the correct answers — which is indeed a good evidence of the Paraguayan attitude toward language, because

it means that the addressees do love and admire Guaraní, but they do not have a thorough knowledge of its cultural value. Only seven questionnaires were filled out and returned to the author, by informants whose personal data are the following:

- No. 1: Schoolteacher, Master of Arts (University of Asunción), 29 years old, female, born in Itá near Asunción, lives in Asunción.
- No. 2: Master of Arts (University of Asunción), specializes in history, 23 years old, female, born and lives in Asunción.
- No. 3: Director of an important government office; Master of Arts, Professor at the University of Asunción, 43 years old, male, born and lives in Asunción.
- No. 4: Diplomatic officer, Master of Arts (University of Asunción), specializes in Educational Sciences, 45 years old, female, born in Presidente Hayes department, lives in Asunción.
- No. 5: Lawyer, 24 years old, male, born in the department of Paraguairí, spent some years in Argentina, Brazil and Uruguay.
- No. 6: Schoolteacher, 35 years old, female, born and lives in the department of Concepción.
- No. 7: Schoolteacher, 31 years old, female, born in Itapúa department (of Slavic immigrant parents), lives in Itapúa department.

The results of these questionnaires will be presented and discussed in the following sections.

## 2. BILINGUALISM IN PARAGUAY

2.1. It is often said that true bilingualism exists in Paraguay, i.e. that Spanish and Guaraní are equivalent or identically frequent in the country, in spite of the fact that Spanish is the official language. Thus, Fogelquist says that "Spanish is the official language of the country, but as a part of his racial heritage, every Paraguayan also learns Guaraní" (1950, 23). Philipson says that Paraguayans are "verdaderamente bilingües" because they speak "dos lenguas con igual perfección, o con perfección casi igual" (1950, 193), which is not true, as we shall see. Most Paraguayan authors speak of the equivalence between the two languages in Paraguay, overlooking the fact that, as we shall try to show, the actual distributional pattern very definitely favors Guaraní. This may be attributed to the desire to show that Paraguay is not an "Indian" country. This should not be necessary today, although it was customary until a few decades ago to neglect or despise Guaraní, which was considered "merely" an Indian language. This former attitude has now disappeared, but it can still be traced through some surviving remains: the pseudo-etymologies of *avañe'ẽ* and *caraiñe'ẽ* (cf. 1.3) are among them. Fogelquist also mentions the fact that "in some educated families parents have been known to prohibit the use of Guaraní by their children" (1950, 23) although we cannot agree with his assumption that this was due to their fear that it would corrupt their Spanish. Common people simply did not speak Spanish in Paraguay until recently, while educated families despised Guaraní as an "Indian" language; the reaction came through a political change (together with the vindication of the memory of Marshal López), but it did not extend to the realization that ONLY

Guaraní is common to the whole nation. Hence the prevailing opinion (among educated Paraguayans) about the existence of true bilingualism.

2.2. Some of the questions in our questionnaire were intended to show the knowledge our informants had of the distribution of Spanish and Guaraní, i.e. the degree of bilingualism in Paraguay. The respective answers are listed below:

A) Question: How long has Guaraní been known in Paraguay?

Answers: No. 1: Since 1609, in times of Hernandarias.

No. 2: Since the Spanish Conquest.

No. 3: It was always known.

No. 4: Since the moment when the first *criollo* (i.e. descendant of Indians and Spaniards) was born, or even before that.

No. 5: Since the Spanish colony.

No. 6: Since the discovery or the founding of Asunción; in 1537, the Guaraní Indians already spoke it.

No. 7: It was always known.

Our comments: Only informants No. 3 and No. 7 gave the obvious answer. Informants No. 4 and No. 6 have a partial and rather vague knowledge of the fact that Guaraní is an autochthonous language, while Nos. 1, 2 and 5 are plainly mistaken. There seems to be a general feeling that the use of Guaraní is somehow connected with Spaniards, probably because this language is AT PRESENT the tongue of the *criollos* or *paraguayos*, who are supposed to be the (mixed) descendants of Spaniards and Indians. This last term is used to denote only the nomadic heathens who once inhabited the country and even now dwell in some regions. It is not a racial or linguistic term, but a cultural one, with a slightly despective flavor. Since there were no *criollos* before the Conquest, there could not have been any Guaraní speakers either. This, of course, is false; but it is, nonetheless, a fairly good evidence of one of the basic attitudes of Paraguayans: bilingualism has always existed in Paraguay since the Spanish Conquest, Guaraní being the tongue of the *criollos* and Spanish that of the conquerors (and of the inhabitants of the other Spanish-speaking South American countries). The question of what language was spoken before the Conquest is usually not even considered.

B) Question: How long has Spanish been known in Paraguay?

Answers: No. 1: Since the time when the missionaries came to carry out the spiritual conquest of the Guaraní Indians in 1609.

No. 2: Since the Conquest.

No. 3: Since the Conquest.

No. 4: Since the arrival of the first Spaniard.

No. 5: Since the Conquest.

No. 6: Since 1537, i.e. the founding of Asunción.

No. 7: Since the Spanish Colony.

Our comments: These answers are, of course, true, except that of No. 1, because Spanish was introduced by the military conquest, not by the Jesuits, who used the native language almost exclusively.

C) Question: What percentage of the total population of the country, taken as a single unit, knows and habitually uses Guaraní?

Answers: No. 1: 97%. The Mennonite and Japanese colonies speak their own language.

No. 2: No answer.

No. 3: 100%

No. 4: 99%.

No. 5: 70%.

No. 6: 100%. All Paraguayans do know and use the Guaraní language.

No. 7: 80%. There are Paraguayans who are descendants of foreigners, and they do not speak Guaraní.

Our Comments: The correct answer would be around 94% (see 2.3). The Mennonites and the Japanese are not the only immigrants in Paraguay. There are also Slavs and Germans, mainly in the department of Itapúa; our informant No. 7 is a descendant of them. Nevertheless, all seven answers are correct in a sense, since actually the divergence reflects diverse attitudes as to who is meant by the term "Paraguayan". The current attitude is that only Guaraní speakers are Paraguayans. This point of view is not shared, of course, by our Informant No. 7, nor by No. 5. None of the answers take account of the non-Guaraní Indians of the Mataco, Guaycurú, Mascocoy and Zamuco tribes, a great number of which still live in the western part of the country (departments of Presidente Hayes, Olimpo and Boquerón).

D) Question: What percentage of the total population of the country, taken as a single unit, knows and habitually uses Spanish?

Answers: No. 1: No answer.

No. 2: No answer.

No. 3: 75%.

No. 4: 80%.

No. 5: About 90% speak Spanish, and the whole population understands it.

No. 6: 75%.

No. 7: I don't know.

Our comments: These answers reveal a characteristic attitude toward bilingualism. The real number of Spanish speakers will be discussed in 2.3, and will be found considerably lower than any of the figures here indicated. This means that no educated Paraguayan could possibly ignore the fact that many of his fellow citizens simply do not know Spanish. Even so, he is unable to make a correct or even an approximately correct estimate, because he actually never or almost never speaks Spanish to anybody outside the higher circles, so he does not know how many people of the lower strata would understand it. Hence the three answers stating complete ignorance, and the other four answers which may be correctly interpreted as expressing only that "not everybody knows Spanish". However, the figures range from 75% to 90% (or 100%, if referred to understanding only), which means that our informants still believe basically that bilingualism is general among the whole population. This cannot be supported by any known facts.

E) Question: Are there any areas in Paraguay where Spanish is not known?

Answers: No. 1: Yes, on the border in the Chaco region.

No. 2: No answer.

No. 3: None.

No. 4: Yes, the Indian tribes.

No. 5: Yes, in the Chaco.

No. 6: Yes, in the Chaco and Emboscada.

No. 7: Yes, the Indian tribes.

Our comments: See under Question F.

F) Question: Are there any areas in Paraguay where Guaraní is not known?

Answers: No. 1: Yes, among the Mennonites in Filadelfia.

No. 2: No answer.

No. 3: None.

No. 4: In the Mennonite colonies.

No. 5: None.

No. 6: In the Japanese, Mennonite and Polish colonies.

No. 7: In the foreign colonies.

Our comments: The answers to questions E and F confirm what we have said about the basic belief of educated Paraguayans that bilingualism is general among the whole population, i.e. that the whole country except the Indian tribes knows Spanish, and the whole country except the immigrant colonies knows Guaraní. They do not seem to be aware of the very evident fact that neither of these groups (the Indians and the immigrant colonies) has any knowledge of either Spanish or Guaraní.

2.3. The answers mentioned and discussed in 2.2 should now be checked against factual data referring to bilingualism, i.e. to actual knowledge of Guaraní, of Spanish, and of both languages. Unfortunately, such information is not available from dependable sources, partly because no official linguistic census has been made, and partly because the partial data available from official sources have been accumulated without any reliable standard of what should be considered as "knowledge" of a language. Thus a child six or seven years old, having acquired some few Spanish words at school, is frequently regarded as a Spanish speaker; so also are all those adults who in their childhood attended school, even if they actually do not remember a single Spanish word and are unable to utter a single Spanish sentence. Taking these circumstances into account, we felt that reliable information should be found before judging the real extent of bilingualism. Therefore we submitted a project in 1962 to a meeting at the Faculty of Philosophy of Asunción, which was intended to carry out a national census of bilingualism with the coöperation of the schoolteachers of the entire country, on a basis of neatly elaborated criteria for knowledge of a language. When, unfortunately, this census was not carried out at all, we planned with Dr. Bertoni, then Chairman of the Academia de la Lengua y Cultura Guaraní, to make a partial study based on data from the general census of 1950. This research would have given us some important information, although the lack of criteria for language knowledge still remained as an important factor. This work could not be finished, because Dr. Bertoni fell sick

shortly after his last visit to Montevideo in February 1963, and he died in August of the same year. However, before falling ill, he managed to finish the first stage, i.e. the extraction of the pertinent data from the records of the Dirección General de Estadística y Censos. These data have been mimeographed by the Academia de la Lengua y Cultura Guaraní, and were sent by Dr. Bertoni to the present writer, in order to plan further research. We reproduce them here:

Department	Population	Only Spanish		Only Guaraní		Bilingual		Other languages	
		Percent		Percent		Percent		Percent	
Asunción	192,259	24,979	13.0	20,307	10.6	146,349	76.1	654	0.3
Concepción	56,248	1,171	2.1	28,802	51.2	26,199	46.6	76	0.1
San Pedro	57,671	1,525	2.7	29,932	51.9	23,586	41.4	2328	4.0
Cordillera	130,299	1,402	1.1	69,590	53.4	59,122	45.4	185	0.1
Guairá	81,506	1,501	1.8	42,833	52.6	36,830	45.2	342	0.4
Caaguazú	64,159	599	0.9	37,961	59.2	24,957	38.9	631	1.0
Caazapá	65,157	756	1.2	34,294	52.6	29,990	46.0	117	0.2
Itapúa	100,118	13,914	13.9	26,368	26.3	55,713	55.7	4125	4.1
Misiones	38,842	849	2.2	16,239	41.8	21,572	55.8	82	0.2
Paraguarí	142,722	2,156	1.5	71,815	50.3	68,400	47.9	351	0.3
Alto Paraná	8,512	173	2.0	3,948	46.4	4,357	51.2	384	0.4
Central	151,186	3,178	2.1	63,320	41.9	84,199	55.9	189	0.1
Ñeembucú	45,676	994	2.1	15,654	34.3	28,982	63.5	46	0.1
Amambay	16,376	726	4.4	7,200	42.9	8,147	49.8	483	2.9
Pte. Hayes	20,954	875	4.2	6,854	32.7	13,151	62.8	74	0.3
Boquerón	25,237	1,959	7.8	5,397	21.4	11,192	44.3	6689	26.5
Olimpo	2,451	130	5.3	705	28.8	1,609	65.6	7	0.3
	1,199,371	56,887	4.7	481,330	40.1	644,753	53.8	16,401	1.4

According to these data, 93.9% of the population knows Guaraní, but only 58.5% knows Spanish. Bilingualism covers only 53.8% of the nation. This is indeed a long way from the answers we obtained to our questionnaires, as discussed in 2.2. If we disregard the capital city, we obtain the following figures for the remainder:

Population	1,007,112	
Only Spanish	31,908	3.2%
Only Guaraní	461,023	46.1%
Bilingual	498,404	49.8%

This means that 95.9% of the population outside the capital knows Guaraní, but only 53.0% knows Spanish. Bilingualism covers only 49.8% of the country beyond Asunción. Yet, although the very regrettable death of Dr. Bertoni prevented us from further direct research, we believe that even these data are greatly exaggerated, and that actual bilingualism and knowledge of Spanish are even less frequent. First of all, the census of 1950 was carried out under very adverse circumstances. Paraguay is a country with very poor means of communication, its territory being partially covered with jungle. There are many settlements in this jungle (for the exploitation of *quebracho*



and *yerba*) which have scarce contact, if any, with civilization. Here Guaraní is spoken almost exclusively. Two large portions of the country, the west and the north-east, are only now being reached by some form of civilized life, thanks to the network of new highways built during the last six years by the present administration. Most of these areas were still completely isolated in 1950. It may be assumed, then, that a great part of the population of these large areas was not contacted in the census of that year. Both areas are almost exclusively Guaraní-speaking; hence we should adjust the census figures to favor the Guaraní language and to lessen the percentage both of bilingualism and of knowledge of Spanish. Obviously, the importance of this factor cannot be indicated in concrete figures. Furthermore, as stated above, there are no standards of what should be called "knowledge of Spanish". It is our conviction that these two factors would make a difference of at least 15% in the total figures. Consequently we believe that less than half of the total population knows Spanish, and less than 40% is actually bilingual. The importance of bilingualism would be still less if real use (*parole*) of Spanish were charted instead of knowledge (*langue*), real or supposed. Paraguayans, even the educated, prefer to address each other in Guaraní, unless the nature of the subject makes Spanish unavoidable (see 4.1). It should be noted also that there is a great difference between the capital, where only 10.6% are Guaraní monolinguals, and the rural areas where Spanish seems to be practically unknown. If we take, for example, the department of Caaguazú, with 64,159 inhabitants, and we deduct therefrom the populations of the two major towns, Coronel Oviedo and San José (totalling 44,244 people), there remain only 19,915 for the rural areas. If we suppose now that at least 60% of the city-dwellers speak Spanish (compare with Asunción, 89.1%), then NOBODY knows Spanish in the rural areas.

2.4. In truly bilingual individuals, there is a clear dominance<sup>1</sup> of Guaraní. In fact, we believe that most Paraguayans are unable to THINK in Spanish, so that when they speak in that language they have to translate mentally from Guaraní. This is, of course, impossible to prove, especially since educated Paraguayans will normally not admit it even to themselves. However, there are some important indications pointing that way. Some of them have been listed by Fogelquist, but we can add some more here. First of all, it is well known that the TEMPO of Paraguayan Spanish is the slowest in the whole Hispanic world. Our personal experience shows that even educated Paraguayans speak slowly, as if they lacked spontaneity. Of the most talented Paraguayan writers, Augusto Roa Bastos, states that, while he writes in Spanish, this always involves the mental effort of translating from Guaraní. Recently he repeated this assertion during an interview in the Argentine newspaper *Crítica*. Now it is very well known to linguists, principally from Ernst Cassirer's work, that language does not involve only the verbal expression, but also a particular analysis of the facts of the universe. When we began learning Guaraní, we had the occasion to experience personally the divergence between Guaraní and Spanish structure, which practically prevents anyone (even a linguist) from learning to speak Guaraní FLUENTLY as a

<sup>1</sup> We attach to this term the sense given in Weinreich, 1953.

second language. The same difference should prevent a native Guaraní speaker from thinking in Spanish, as stated by Roa Bastos, despite the fact that he is in perfect command of Spanish. We have had the occasion to notice, during one of our lectures at the University of Asunción, that while our lecture was delivered in Spanish, some students were taking notes in Guaraní. This clearly means that it must have been easier for them to translate mentally into Guaraní and THEN to abstract, rather than to abstract directly in Spanish.

2.5. All this leads to the conclusion that Paraguay is not really a bilingual nation, but a Guaraní-speaking country where, on the higher levels of administration, education and wholesale trade, Spanish is used out of necessity, because of the lack of serviceability of the native tongue (cf. 4.1), as well as for historical reasons. This second language is learnt by all individuals of the higher strata and, on the lower strata, by those who cannot avoid contact with official activities, but only to the extent required by those activities. Only a small elite uses it even in everyday private life.

### 3. SOCIAL STATUS OF GUARANÍ

3.1. All Paraguayans realize that the Guaraní language is the most genuine manifestation of their being an independent nation. As we have already seen, they do not consider anyone as Paraguayan who fails to master the tongue. There is an attitude of complete identification between the language and the nation itself. This feeling goes so far that the average Paraguayan, even the educated one, very often believes that there could have been no Guaraní language before the founding of the nation, or before the racial merging which gave rise to the *criollos* (see 2.2). Thus, without having reached the stage of a standard language in other aspects, Guaraní fulfils the three symbolic functions of a standard language as enumerated by Garvin and Mathiot (1960, 785): the unifying function, the separatist function and the prestige function. We should prefer to speak of symptomatic, rather than symbolic, functions, both because they do not emanate from the symbolic nature of language and because they do not depend on the level of standardization. Even so, we may distinguish two kinds of prestige function, one from the standpoint of the whole nation as opposed to the neighboring nations, and the other as referred to the individual within the nation. This second function corresponds only to the standard language and will be discussed in 3.3. Here we can state that, unlike the case in other South American countries where native languages are still being spoken, in Paraguay the ability to speak Guaraní either as the mother tongue or as a second language, far from being a handicap for social advance, is even a necessary condition for being considered a part of the society. Social advance is highly dependent upon the knowledge of Spanish, while Guaraní apparently is not taken into account in this respect. But actually it is the very basis of the whole process, since social distinction is made between those who speak only Guaraní and the bilingual, while those who speak only Spanish have no social rating at all.

3.2. An important manifestation of the national feeling is the national pride which is embodied in the language. This becomes evident through countless expressions of Paraguayan writers. Surprisingly enough, while most Paraguayans believe or feel vaguely that Guaraní has been spoken only since the Spanish Conquest, on the other hand many of them say that it was as important, before Columbus, as Nahuatl, Maya or Quechua — or more so. We personally believe that its present importance is most certainly greater than that of those other languages. This, however, could not be said of the times prior to the Conquest. Notwithstanding this, Morínigo's very objective work (1931) met sharp criticism in this respect in his native country. Paraguayan scholars often try to demonstrate the extent of their language's influence by finding Guaraní etymologies for place names all over the continent. Jover Peralta (1950, 187-188) has "found" eleven place names of Guaraní origin as far away as the United States (all of these etymologies are completely ridiculous, as for example *Ticonderoga*, in New York state, interpreted as Guaraní *tenondé* "before" plus *roga* "house", which would not even be grammatically correct in Guaraní). Solari (1928) explains the name of the *Amazonas* river from Guaraní *amá* "rain" plus *sunú* "sound", disregarding the very well known history of this name. Etymologizing of Guaraní or pseudo-Guaraní place names has always been a favorite hobby, with apparently no limits to one's fancy, as has been shown in a paper of ours (Rona, 1960). In other respects, too, we find the same fantastic attitude of pride: we once heard, during a lecture by a well-known professor whose name we do not want to recall here, that the greatness of Guaraní is comparable only to French, "because both are oxytone". All Paraguayans agree that Guaraní is a "great" language. In this exaggerated pride we see a typical psychological reaction, because its real meaning is that ONE MUST NOT FEEL ASHAMED of speaking Guaraní. This is true, of course, because no one should feel ashamed of his mother tongue. However, this fact need not be emphasized to such an extent. As everywhere, any startling divergence between a fact and its interpretation can be evaluated in terms of attitude. This particular instance shows a very typical attitude toward the social status of a language which had been formerly considered as "merely Indian", and whose speakers were often socially despised (cf. 2.1).

3.3. Is there any difference between a "vulgar" and a "cultivated" Guaraní language? In other words: is there a socially significant standard Guaraní? The first question has been included in our questionnaire. We obtained the following answers:

- No. 1: Yes, there is a vulgar and a cultivated Guaraní. The difference consists in the use of Spanish words.
- No. 2: Yes, there is a difference. I could not quote concrete cases.
- No. 3: Yes. Present-day speech is considered vulgar because of the invasion of Spanish words.
- No. 4: I don't know. I know only that there is a "pure" and a "Hispanized" Guaraní.
- No. 5: Yes. Standard requires the use of the Guaraní word, not the "Hispanized" one.
- No. 6: Yes. There are obscene words, frequently used by vulgar people.
- No. 7: Yes. I could not quote any.

The answers of this set speak for themselves. They all (except No. 6) refer to the problem of the so-called *jopará*, i.e. the mixing of unnecessary Spanish words into Guaraní sentences. This is a very common habit among all Paraguayan speakers, in part due to the fact that the native language is not serviceable enough (see 4.1) and lacks words for many common concepts. None of our informants comments on the existence of divergent language forms, which we have not found in Guaraní. *Jopará* itself may occur more or less frequently, but it cannot be completely avoided. The manner of speaking Guaraní must therefore be considered socially irrelevant, because speakers do not notice differences, whether they do or do not exist.

3.4. One more question arises in connection with the social status of the language from the symptomatic viewpoint. We asked our informants to tell us whether there were dialectal (geographical) divergences within Guaraní. The answers to this question were the following:

No. 1: Guaraní is uniform.

No. 2: I don't know.

No. 3: There are different pronunciations.

No. 4: Guaraní is uniform.

No. 5: Guaraní is uniform.

No. 6: There are several dialects: the *guaireño*, the *misionero*, the *encarnaceno*.

No. 7: It is not uniform. In the central region, pure Guaraní is spoken, while here it is mixed with many Spanish words.

In fact, we have noticed various dialects during our trips through Paraguay. They are due in part to the influence of the Jesuit fathers, which affected the Western and central parts of Paraguay to a lesser degree than the east (Misiones). The differences are, however, unimportant and difficult to notice for an observer not trained in phonetics. Apparently the country schoolteachers (informants Nos. 6 and 7) can notice it better than the *asuncenos*.

#### 4. CULTURAL STATUS OF GUARANÍ

4.1. Most Paraguayans usually assert that their national language is especially fit to express abstract notions, and that it has a suitable word for any concept. Thus Guasch (1950, 231) states: "Apenas se hallará alguna palabra castellana que no tenga o pueda tener su equivalente en guaraní". It is well known to any linguist, and it was officially admitted at the Inter-American Indigenist Congress of Pátzcuaro in 1940, that any American Indian language can be developed to a degree where it might serve to express any idea. The effectiveness of a language is, however, not God's gift, but the result of a long process of hard work and technical knowledge. Nothing could be more dangerous for this process than the *a priori* belief that a language has been "perfect" since remote times. Such is the danger, however, which lies in the attitude of Paraguayan speakers of Guaraní. The preceding assertion of Guasch should be compared with the

bare fact that Guaraní has numerals only from one to four. From five on, numbers are expressed in Spanish. Several numeral systems have been proposed, but none of them has met with general approval, not even among scholars. The average educated Paraguayan thinks that he can speak in his native tongue about any subject. Actually, when a difficult subject turns up, he simply shifts to Spanish, sometimes without even realizing it; or he continues speaking in Guaraní, but his sentences are spattered with Spanish words. This then is *jopará* on a higher level. We paid due attention to this circumstance in our questionnaire, and included the following question: Are you able to speak in Guaraní on a scientific subject? The following answers were obtained:

No. 1: Yes, OF COURSE.

No. 2: No.

No. 3: No.

No. 4: Yes.

No. 5: Yes, with a certain difficulty in the choice of proper words.

No. 6: Yes.

No. 7: No.

Some of the negative answers were written down only after having read the third section of the questionnaire. This section contains Spanish sentences in a gradual order of difficulty, which were to be translated into Guaraní by the informant. The answers to this section will show the actual lack of serviceability of Guaraní. The first sentence was *Vengan Vds. a visitarnos* "Please come to pay us a visit", the correct Guaraní equivalents of which are extensively discussed by Morínigo (1959: 244 ff.). Our informants translated:

No. 1: Peyú mí oré rechavo.

No. 2: Peyú che rechavo ("me" instead of "us").

No. 3: Peyú mí oré rechavo.

No. 4: Peyú mí ke na oré visitá (*visitá* is Spanish).

No. 5: Peyú oré andú (*andú* from Spanish *andar*).

No. 6: Peyú peé che rendape.

No. 7: Peyú oré visitavo (*visitavo* is from Spanish).

The next sentence was: *Cuando este niño haya crecido, será un buen carpintero* "When this boy has grown up, he will be a good carpenter".

No. 1: Co mitá ocacuaá vo vé oicó vaerá ichugüí carpintero porã.

No. 2: I carιά i riré co mitá, oicota chugüí, petem carpintero porã.

No. 3: Co mitá i carιά i maro, carpintero porã vaerá.

No. 4: Agã tuvichá vo vé co mitá osêne chugüí carpintero porã.

No. 5: Co mitá ocacuaá haorá yvyrapandara porã ta.

No. 6: Co mitá tuvichá haorá oicó vaerá chugüí carpintero valé.

No. 7: I don't know.

Here only one of our informants uses the Guaraní word for “carpenter”, *yvyrapandara*. One informant did not translate.

The next sentence was: *El cometido primordial de la Universidad consiste en crear un ambiente adecuado para el progreso de la Patria*, “The foremost duty of the University is to create an environment suitable for the progress of the country”. This should not be a difficult sentence, especially since all the informants are educators. They translated:

No. 1: Icatú haguaichá opuá ñané retã Aranduroga oyapó vaerá tamiapó porã.

No. 2: I don’t know

No. 3: Impossible.

No. 4: Pe oga-guazú ñandé mboéva o meé ñandere pyá-guazú ña mo tuvichá haguá ñandé retã.

No. 5: Only a member of the Academy could translate this.

No. 6: Tecó te vé co arandú meé haoné haã mbareté o mo puá haguá ñané retã.

No. 7: I don’t know.

Here, four informants did not translate. The three translations are not concordant, and only one of them uses the correct term for “University”, which is *Aranduroga*.

The next sentence was found impossible to translate by all seven informants, even by No. 1 who had stated that “of course” she was able to speak in Guaraní about any scientific subject: *La fisión de las partículas nucleares determina la liberación de elevados niveles de energía nuclear*, “Fission of nuclear particles releases very high levels of nuclear energy”. Our next sentence was: *La interpretación de las relaciones de motivación es, en su esencia, una problemática de integración filosófica*, “The interpretation of the relations of motivation is essentially a problem of philosophical integration”. Only informant No. 6 managed to translate this sentence, and even she did it incorrectly. She omitted some of the scientific terms and used some Spanish words, so that no single Guaraní philosophical term appears in her translation: *Narutendé ke haé hã científica, ha aveí filosófica*, which is not a translation of our sentence. All other responses were “I don’t know”. Finally, the informants had to translate a very simple mathematical statement: *La raíz cuadrada de cuatro es dos*, “The square root of four is two”. Informants Nos. 1, 2, 3, 5, 6, and 7 were unable to translate, while No. 4 wrote *Mocoi hina raiz cuadrada irundygũi*, which means that she did not know the Guaraní expression for “square root” either. According to Guasch (1956, 88), the correct answer would be *mocoi hina irundy rapocoi*. This shows that, despite the claims of all informants that they usually look up difficult expressions in the grammars or dictionaries of Guasch, Osuna, Jover Peralta et al., the fact remains that the efforts of the Academy and other learned societies and scholars to give Guaraní a greater efficiency are not paralleled by any conscious effort of the average educated Paraguayan, who prefers to use Spanish words in the midst of a Guaraní sentence, or simply to switch to Spanish whenever he finds some difficulty in Guaraní. This evidently contradicts the assertion of Garvin and Mathiot’s informants (1960, 789) about the feeling for the desirability of a norm, as well as that about the high regard of literate Paragua-

yans for the work of the Academia de Cultura Guaraní. We could state instead that the frame-of-reference function of standard Guaraní simply does not exist.

4.2. Our personal experience shows that even educated Paraguayans are only "naïve" speakers of Guaraní; to put it in Eugenio Coseriu's words, they have only practical, not theoretical knowledge of their language. Until not long ago, Guaraní was not taught at all. Some good, or not so good, dictionaries have been published; but grammars are absolutely deficient, in the first place because they reflect the grammarians' own ideas of Guaraní, not the Guaraní language itself, and secondly because of their excessive latinomorphism or hispanomorphism. Thus, according to Osuna (1950), the Guaraní verb would have the following tenses: *presente, copretérito, pasado próximo, pasado general, pasado remoto, antepretérito, antecopretérito, futuro, pospretérito, antepospretérito*. Only the above-mentioned grammar of Guasch (1956) avoids falling into the second misconception, but the author uses instead a completely different grammatical terminology, which would not be understood by a reader who had not previously learnt the general theory of grammar. There is not the slightest agreement between the various authors in any respect. Consequently there cannot be a language standard acceptable to the common speaker. In such circumstances, the introduction of Guaraní into educational plans has not improved the situation, simply because there is no material sufficiently good for the teaching of the language. Educated Paraguayans do not know anything about the teaching of Guaraní. We asked our informants three questions on this subject in our questionnaires: (1) Is Guaraní being taught in primary (elementary) schools? (2) Is it being taught in secondary schools? (3) Is there a chair of Guaraní at the University of Asunción? Our informants answered as follows:

	(1)	(2)	(3)
No. 1	No	Yes	Yes
No. 2	No	No	Yes
No. 3	Yes	Yes	Yes
No. 4	No	In some cases	I don't know
No. 5	No	No	Yes
No. 6	No	Yes	Yes
No. 7	No	In some cases	I don't know

When asked whether they had read any Guaraní grammar, only three informants answered affirmatively. On asking them which grammar is used at school and whether there are any Guaraní textbooks, we obtained the following answers:

	To the first question:	To the second question:
No. 1	According to the teacher's preference.	Yes.
No. 2	I don't know.	No.
No. 3	None.	No.
No. 4	I don't know.	I don't know any.
No. 5	It is under study.	There are several.

No. 6 I don't know.

No. 7 I don't know.

Yes.

I don't know.

Actually, there is no Guaraní textbook. Informant No. 5 surely took grammars for textbooks in answering our question.

4.3. Normative activity should be performed by the Academia de la Lengua y Cultura Guaraní, which has been established for this purpose. This has, however, not been carried out as yet. All informants knew about the existence of the Academia, but they were unable to point out correctly its purpose and its work. This means that (1) The Academia does not influence the cultural status of Guaraní; (2) educated Paraguayans are certainly proud of their having an Academia; but (3) they do not care to apply or even learn its findings and its standards, if any. This explains why the informants of Garvin and Mathiot were unable to give adequate information. The responses of all educated Paraguayans are similar in this respect, and only further questions and checking of the answers will reveal their falseness. Our questionnaires contain two questions concerning this item. After some introductory questions relative to the mere existence of the Academy, its year of founding, its present authorities, etc., the first pertinent question is: What are the activities of the Academia?

No. 1: Diffusion of the various aspects of Guaraní: cultural, social, etc.

No. 2: I don't know.

No. 3: Study of the language and of the culture (ceramics, religion, mythology), as well as delivering lectures.

No. 4: Conservation, purification and spreading of the Guaraní language.

No. 5: Study of the language and promotion of its use.

No. 6: Study of the possibilities of official use of the language.

No. 7: I don't know.

The second question was more specific: Name some of the publications of the Academia.

No. 1: Bulletins, religious texts for the peasants, broadcast programs, newsletters of the U.S. Embassy translated into Guaraní.

No. 2: I don't know any.

No. 3: Folders or newspaper articles, published sporadically.

No. 4: I don't know any, but I'm aware that they have some excellent publications in the form of folders, magazines, etc.

No. 5: There are folklore magazines, some newsletters and catechisms.

No. 6: I don't know.

No. 7: I don't know.

To the question, "What do you do when you doubt whether a Guaraní word or grammatical form is or is not correct?", two informants answer that they consult some professor of the Faculty of Philosophy (which, of course, cannot possibly be done by every Paraguayan); the other five say they look it up in some Guaraní-Spanish diction-



ary or Guaraní grammar (but three out of these five answered to another question that they had never actually read a Guaraní grammar.)

### 5. CONCLUSION

From this discussion, we must conclude:

(i) that there is no true or general bilingualism in Paraguay, but Spanish is used, mostly for administrative and educational purposes, by no more than half of this Guaraní-speaking nation;

(ii) that Guaraní fulfills the so-called unifying and separatist functions;

(iii) that no social relevancy is attached to Guaraní in Paraguay, this function being reserved to the knowledge of Spanish as a second language, but those who do not know Guaraní have no social rating at all;

(iv) that Paraguayans are extremely proud of their language and feel the necessity for developing it into a vehicle of culture, but they simply do not know how to do it. Our final opinion is that they deserve help and good counsel for this task, from scholars the world over.

### DISCUSSION

MATHIOT: Since Rona has mentioned the paper by Garvin and Mathiot (1960), I would like to make a few comments as to how our material was gathered. I think it was some time in '54 or '55 that I personally conducted interviews with Paraguayans whom I had found in Washington, D.C., and in the course of a trip to Michigan. All of the informants had come to this country to study English and consequently were of more or less the same educational level. One of them was a doctor; most of the others, I think, were in the educational system of Paraguay. In addition to interviewing informants, I consulted all the original material that was available in the Pan-American Library in Washington. I looked at Paraguayan periodicals to see what was said about the importance of Guaraní in Paraguay.

However, Rona again and again makes the same critical comment that one should not trust one's informant. To give an illustration of why one should not trust one's informant, he uses the case of bilingualism. He shows that when he asked his informants direct questions as to who can speak Guaraní in Paraguay, he got one picture; but when he took the census figures, he got a completely different picture. From this, i.e. from the discrepancy between the two patterns of data, he concludes that one is a false image and one is a true image. I think he is wrong in this conclusion. Both patterns are very important, and each should be taken into consideration, especially when one studies attitudes. I think that Kluckhohn's distinction between ideal patterns and behavioral patterns applies here; and this conceptual distinction is extremely important, because the amount of coincidence or discrepancy between the two patterns allows one to interpret the picture.

What we worked on, of course, was the ideal pattern. What Rona worked on was the behavioral pattern. This is emphasized by the fact that he conducted his interviewing in Paraguay and used special questionnaires to find out what he calls "the true facts".

P. Ivić: There is a very significant place in the paper where the author mentions that the public ridicules everybody who tries to speak Spanish. This is a point important for the study of the attitudes of the public. Wherever two languages co-exist in a social hierarchy (higher vs. lower), there are two possible attitudes. One of them corresponds to pressure from above in the sense used by Labov, and the second to what I would like to call pressure from below, in a sense different from Labov's. This kind of pressure usually prevails first after the penetration of the standard language in a rural ambiance. The original situation is still too strong, and peasants laugh at those who want to speak like town people. But after a certain time, maybe after several generations, the struggle between the dialect and the standard language reaches its turning point. The prestige and the general usefulness of the standard language are already well-established, so that now it is those who don't know how to speak the town language who are ridiculed. A rapid decline of the dialect begins. Thus the direction of the derision appears to be characteristic for the attitude of the community towards the two linguistic patterns, and the derision itself plays a powerful role in the struggle between those patterns.

FERGUSON: This situation in Paraguay seems to be a unique pattern of bilingualism on a national scale in the world, and so calls for repeated study. It differs in many respects from the standard-versus-dialect situation that Professor Ivić was referring to, because it has been a stable bilingualism for some centuries now and doesn't seem to be moving in either of the directions that he's talking of: it seems to have a different set of factors. It is interesting not only because it seems to be unique in this way, but also because the history of the situation is fairly well known; so it is possible to make hypotheses as to why it worked out this way and not some other way, and then to work from that to other cases where we can find partial similarities.

I also want to add a little ammunition here on the discrepancy between the informant's evaluation of the situation and the actual situation. There were two students of mine who worked on a term paper about the situation in Paraguay: one based all his work on interviewing a single informant from Paraguay, and the other based her work on having lived there for two years. The pictures that resulted were totally different. If one had believed the information that had come from the informant, had accepted it at face value, one would have been very seriously misled. Obviously, of course, you could handle this in a different way; that is, you could look at how this must reflect certain attitudes and values, and so on. I am just emphasizing the great discrepancy in this particular case between the two kinds of information.

MATHIOT: I don't like your term "misled" because I don't think you should "believe" an informant, but rather accept his responses as data. It is not a question of believing or disbelieving, but of knowing how to interpret one's data in order to reach a conclusion.

M. Ivić: I would like to add an observation about the influence of nationalistic feeling in some bilingual communities. When the cultural prestige belongs to the language of the politically dominating social group, whereas the politically oppressed group speaks the vernacular, such a vernacular may easily get another kind of prestige. I was told by Blaže Koneski, the founder of the Macedonian standard language, that as a young boy, returning to his Macedonian native village from the Serbian town where he went to school, he was ridiculed for his Serbianized language. Then he started to re-learn the vernacular to be sure that he did not betray the national feeling of his own people. The formation of such an attitude is a necessary prerequisite to the nationalistic trend of establishing the vernacular as a new standard language.

LABOV: One issue raised by this paper is how to make use of questions on overt attitudes towards languages taken as a whole. It might be useful for me to just mention the questions which we asked of New York City speakers about their general attitudes toward language. First, we asked them, "What do you think of New York City speech?" in order to get their salient reactions. "New York City speech" did appear as a single identifiable item in their subjective reactions. Then, "Have you ever traveled outside of New York City? Did anyone recognize you as a New Yorker by the way you spoke?" And, "If so, do you think that they liked New York City speech? If somebody said that you talk just like a New Yorker, would you take it as a compliment?" We then asked, "Have you heard any southerners talk? Just thinking of the sound of southern speech, would you say you like it better than the sound of New York City speech or not?" Then we shifted to questions of correctness: "Can speech be too correct?" All these questions were asked after all data were gathered to measure the informant's own speech performance, and the responses to these questions were no more indicative of any factual situation than responses to self-evaluation or any other subjective response. But they did prove to be extremely helpful in comparing subjective evaluation: for example of Negro as compared to White informants, of people who were raised in New York City as compared to people who were raised out of town. Our results indicate that these attitudes can be extremely useful in understanding objective performance, if you adopt the attitude that has been set forward here — that these responses are data in their own right and must not be looked at in any other way.

RONA: My paper on the social and cultural status of Guaraní in Paraguay is only a quick glimpse of a longer and more complex research project which I have been conducting for some time and which will eventually be the subject of a book. Like most papers submitted to symposia, it is intended to show my colleagues one particular aspect of my research work, pointing to one particular result. In this case, during my research I realized that the measuring of attitudes towards language could be a good research method (a new method, if I am not mistaken) in sociolinguistics. This was the subject of my paper, as specifically pointed out in the introduction.

When I speak of 'attitude', I do not mean 'belief' or 'thought', and obviously I do not mean 'facts of language or of the use of language'. I think that there is a very

clear difference between 'belief' and 'attitude'. The first can be defined in itself, while the second is meaningful only if compared with actual facts. We may believe in God, and this is only a belief, not an attitude towards God, because nobody KNOWS of His existence. Other persons might not believe in God, and this would not be an attitude in itself. If somebody says, "My country is large", this is merely a belief, if we do not know where he comes from. Now if an American and a Uruguayan say, "My country is large", this statement reveals two very different attitudes, because, of course, the United States is large, but Uruguay is NOT. We might deduce therefore one attitude for the American, and a quite different one for the Uruguayan. Thus, the same belief may be a part of two different attitudes. A belief is not an attitude if not referred to the facts corresponding to its subject, any more than an utterance is a linguistic sign if not associated with a meaning, and a man is not a husband if not referred to a wife. Thus, the utterance [sal] is part of quite different language signs in Spanish ("salt"), French ("dirty") and Hungarian ("stem"), and it is not a language sign at all for an English-speaker or for an Italian, where no meaning is attached to it. It is easy to note the parallelism with the case of the statement "My country is large". If anything, my conception of attitude could be compared with De Saussure's conception of language sign:

belief
facts

= attitude

Thus, we have three, not two sets of data. Two of them are primary, and therefore they can be obtained only through research (but there should be a different research method for each), while the third set must be deduced from the primaries.

Now the second set of data may involve facts from any field of human activities, or even from non-human activities. Attitude is therefore a psychological phenomenon. It is within the scope of linguistics (or sociolinguistics) only if the factual set refers to language or to the use of language. The 'belief set' in itself could not possibly show us either the language nor the use of the language.

I would call these three sets of data, if referring to language or to the use of language:

1. Belief facts (these are the "attitude facts" of Dr. Mathiot).
2. Language facts.
3. Attitude facts.

As mentioned by Dr. Mathiot during the discussion, there are two patterns corresponding to 1. and 2., and attitude may be determined by comparing those two patterns.

Speaking now of the use of Guaraní, I notice that all authors who have written about this subject worked with only one set of data, that of the 'belief level' (except Morínigo, who being himself a Paraguayan, knows many of the 'language facts'). Drs. Mathiot and Garvin have fully realized this circumstance, and they presented

their information as belief, not as language facts. That is why I fully agree with Dr. Mathiot's observations about the coincidence of their work with my own. She most acutely and accurately points out that there are two different patterns, which may show coincidence or discrepancy, but nevertheless both are FACTS on their proper levels. This is very clear to me. Other scholars who wrote about the use of Guaraní, such as Fogelquist, presented the 'belief data' as a description of the bilingualism, i.e. as if they were 'factual data', which is not correct. My own paper contains the two sets of primary data, and it is aimed to show the divergence between them, which I feel is a good basis for judging or measuring attitudes.

It is very interesting to see that Dr. Ferguson's students, having the same two sets of data I had, arrived at the same situation I did. This, I dare say, is the best confirmation of my claim that the attitudes are extremely interesting, and we need a more careful study of them. Paraguay is an unusually rich field for research, as most colleagues pointed out at the Conference, but I think that my results indicate that the method should be applied to other situations as well (e.g. to the situation in India, mentioned by Bright).

All I did in this paper was:

1. Point out my intention to compare 'belief facts' with 'language facts' for the study of attitudes;
2. Present both sets of data and compare them;
3. Draw (I think) valid conclusions; and
4. Show that this method works.

On the other hand, a procedure which I do not approve of and do not follow has been constantly used by all those scholars who wrote about bilingualism in Paraguay (except Mathiot and Garvin). In fact, all those scholars describe bilingualism on the basis of what their informants, or their source scholars' informants, had told about what percentage of the population of Paraguay speaks Guaraní and Spanish. If I am not mistaken, I have been the first in checking the informants' data against true language facts, because I was the first in having those data available. Now why should I leave out a question everybody asks, if I can check the answers better than anybody else could?

One last word: it has been said that if you can not trust your informants, you should not ask them questions. This is true, but only when you ask them about the language itself. Every speaker has what Coseriu calls a "technical" or "practical" knowledge of language itself, although he seldom has a theoretical or scientific knowledge of it. But when dealing with social aspects of language, then the assertion we are commenting upon is not always true. Speakers almost never have even a practical knowledge, so that the enquirer should deduce indirectly the bare facts. You may ask, "How do you say when a liquid is easy to set on fire?" and you may trust your informant without any skepticism: you may be sure that, if the answer was "flammable", then this particular speaker (a typical speaker, of course) uses that word. This is linguistics. But you should not accept that easily an answer to the

question, "Is the word *flamable* correct (or standard, or elegant, or beautiful, or pleasant, or educated)?" This is sociolinguistics. The enquiry methods of one cannot always be trusted for the other. In the second case, you should submit the answers to further analysis, and very carefully indeed. If a majority says that *flamable* is standard English, this will not prove that it is; but it will show that the majority does not know standard English, which is a most valuable conclusion in sociolinguistics.

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## THE STATUS OF HINDI AS A LINGUA FRANCA

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As a political experiment, the Indian Union bears comparison with a United States of Europe. One of the major threats to the success of the experiment lies in the large number of regional languages which serve as a focus of cultural and political loyalty below the national level. The constitution recognizes as official fourteen languages from two major linguistic groups. Of these, Marathi (in Maharashtra state), Gujarati (in Mahagujarat), Punjabi (Punjab), Hindi (Punjab, Uttar Pradesh, Madhya Pradesh, Rajasthan and Bihar), Bengali (West Bengal), Assamese (Assam), Kashmiri (Kashmir), Oriya (Orissa), Urdu (in the Hindi speaking areas and sporadically throughout the country), and Sanskrit are Indo-Aryan. Telugu (Andhra), Tamil (Madras), Malayalam (Kerala), and Kannada (Mysore) are Dravidian. All of these languages, with the exception of Punjabi, Kashmiri and Sanskrit, have more than ten million speakers, and even Sanskrit, according to the Census of 1951, has 555 native speakers, with many people throughout the country reporting it as a second language.

The high correlation between linguistic and political boundaries represents in good part the successful application of local pressures for the reorganization of state boundaries along linguistic borders in the nineteen-fifties, much against the inclination of the Congress Party nationally, which correctly foresaw the political danger of these moves.<sup>1</sup> Mahagujarat was carved out of the old Bombay state; Andhra was created from the bulk of the territories of the Nizam of Hyderabad and the Telugu speaking districts of the old Madras state. Bombay (Maharashtra), Mysore, and Madras respectively acquired Marathi, Kannada and Tamil speaking districts of the Nizam's former territories. Agitation, notably in the Punjab and in Madhya Pradesh, continues.

In addition to the official languages, there are three languages (exclusive of local or caste names for varieties of Hindi) whose mother-tongue (MT) speakers number in the millions — Santhali, Gondi, and Bhili — and several smaller languages whose speakers play an important role in the life of the Union. Perhaps most notable among these last are the 750,000 MT speakers of Sindhi, mostly displaced from West Pakistan at the time of the partition, who have achieved considerable power, particularly in the economic life of Bombay and Delhi.

<sup>1</sup> For full discussions of these problems, see Harrison 1960 and Friedrich 1962.

A nationalist buffer to this linguistic diversity, with its largely areal base and attendant political parochialism, is the felt need for a national language. The Constitution specified that Hindi was to become the official language of the Union by 1965, but various hedges have since been made, the most recent contemplating the continuance of Hindi as an "associate" language beyond the 1965 deadline. The national language issue was in part a by-product of the struggle for independence, an attempt to give a linguistic analogue to the theme of cultural unity which the Indian National Congress stressed in its campaign for nationhood. The way however has been hard. As late as 1956, some time after the emotional peak reached in the Constituent Assembly, which wrote Hindi into the Constitution as the official language of the Union, the Official Language Commission recommended that a change-over to Hindi in all aspects of government be made immediately. It took the position that 42% of the people already knew Hindi and that, since it is closely related to many of the other regional languages it would be relatively easy for speakers of those languages to acquire. (This argument, of course, has no appeal to speakers of Dravidian languages.) In advocating Hindi the Commission majority noted that there were two varieties, one with a relatively high percentage of Persian vocabulary and one with a relatively heavy infusion of Sanskrit, thus skirting the Hindi-Urdu problem, but made no attempt to deal with the wide range of geographical dialect variation nor with the competing claims of regional standards which are quite different, particularly in morphology, from the *khari boli* or "Delhi standard" which the Committee recommended. Many of these dialects have considerable literatures, and local loyalties split Hindi speakers on the question of what constitutes "standard Hindi".<sup>2</sup>

At the moment it is probably fair to say that no one is a speaker of standard Hindi. Educated speakers learn a variety of Hindi from which as many localisms as possible have been edited, so that their careful speech approximates as closely as possible the usage of the variety of *khari boli* employed in modern prose. (This is true of morphology and syntax but is less likely to extend to phonology.) In vocabulary, where much of the attention given to the standardization or purification of Hindi has been focussed, the situation is vexed. There have been determined efforts on the part of linguistic and political conservatives to purge the language of all English and Persian loans. As might be expected, these have been largely unsuccessful, although some innovations, propagated by the All India Radio, are at least understood. Common Portuguese loans are less visible and have received less attention. In fact, most educated speakers of Hindi are multi-style speakers, with a Sanskritized style appropriate to formal situations, and a Perso-English style for others. Generally, highest frequency for English loans is in the informal speech of the Western-oriented educated speaker and in lower-class speech. Highest incidence of Persian loans is among the linguistically un-self-conscious middle class. The Commission, with a pious hope that extremes will be avoided, remarks: "Whatever the extent of these dialectal distinctions in Hindi amongst us, there will have to be a core of standard literary Hindi, based upon the

<sup>2</sup> Careful analysis of these is given by Gumperz and Naim 1960.



'khari boli', and accepted everywhere and admitting only of variations of style. It is this Hindi, which is based on what is understood by the common people, and which is in some measure already a 'lingua franca,' being more or less understood on Railway Stations and at places of pilgrimage where the Indian people drawn from different linguistic regions foregather, which has been appointed to be the language of the union by virtue of the large numbers of people who know it already..." This statement seems to link *khari boli* with what is usually called Bazaar Hindustani. While few would want to agree with this link, many would agree that if there is now in India a lingua franca other than the English of the educated elite, it is the bazaar pidgin version of Hindi.

One may also note that the attempt to extend the domain of any variety of Hindi beyond its proper regional borders, even if it is done in the name of national pride and the need for a native language common to all the people, is seen as having the effect of promoting the regional interests of the Hindi-speaking people. The strong feelings of the non-Hindi-speaking people were voiced in two bitter dissents from the report of the Official Language Commission, a minority report filed by S. K. Chatterji, a Bengali, and a note of dissent by P. Subbarayan, a Tamilian. Unappeased by the advocates of Hindi who assured the non-Hindi speakers that the language of the future would not be the regional language, and that the Hindi of the future would draw on all the languages of the Union to "enrich itself", Subbarayan noted that a language cannot be developed to order. (The notion of such development seems to be rife in the Commission's report and in much of the discussion of the language problem in India. Indeed, the enrichment of Hindi by an infusion of vocabulary from various Indian languages appears to some an irresistible analogy to the infusion of capital into a developing economy.) Both Chatterji and Subbarayan show the strong linguistic chauvinism which is particularly characteristic of (or at least particularly well articulated by) speakers of Bengali and Tamil. Both languages have long, rich literary heritages, and appeals to these heritages evoke strong emotions. Less emotional but very important is the fear, expressed by Chatterji, that the establishment of Hindi would mean more or less permanent second-class citizenship for all non-Hindi speakers, particularly if examinations for government jobs were given in Hindi. This is a real issue. While it is true that competition for such jobs involves a very small proportion of the population, it involves a large proportion of the politically activist intellectual elite, for whom (and for this reason) the language issue is more than an emotional one.

With this background, I would like to examine the status of Hindi as a lingua franca outside the regions in which it is the dominant language, by considering the patterns of bilingual behavior in several major cities with multi-lingual populations, where pressures for a lingua franca may be assumed to be greater than in rural areas. My information is derived from the 1951 Census (1961 figures have not yet become available to me). A table of percentages is appended.

The Census asked only for returns of second languages in daily use, and therefore

does not provide any measure of actual second-language knowledge or of multilingualism. Furthermore, no estimates of the use of English as a lingua franca can be made directly, since only use of a second Indian language was reported. I will attempt, however, to make some inferences from the reported bilingual behavior of mother-tongue speakers of English. The Census of India has never reported the use of English as an other tongue (OT); and reports of multilingualism no doubt would be very difficult to handle and would in any case be misleading, since there could be no check on the degree of control of the second languages. Nevertheless, it seems to me that these conditions tend to maximize reports of Hindi as an OT, including everyday use of the pidgin variety. Skirting the entire internal problem, I have treated Hindi and Urdu MT reports as the same mother tongue, and I have discounted reports of Hindi as an OT reported by MT speakers of Urdu or vice versa.

Of the four major cities which lie outside the Hindi heartland, only Bombay, linguistically the most diverse, lacks a statistically dominant language. (See the table on p. 305.) Telugu, with 53% of the population of Hyderabad, barely meets this criterion, but it is likely that the new census will show an increase in the percentage of Telugu MT speakers in the city. There are parallels between Calcutta, Hyderabad and Madras in that MT speakers of the two most widespread languages learn as OT the language of the other group. Pressures to learn, however, vary greatly. In Hyderabad, where the majority of Telugu is precarious, the highest percentage of bilingual behavior is returned. In Calcutta and Madras, where there are clear majorities for Bengali and Tamil, there is less, with the 4% figure in Calcutta providing a minimum for all the cities. English MT bilingual behavior is comparably high in Madras and Hyderabad, but much lower in Calcutta. The information on the continued use of English as a lingua franca, which we might expect to find in the English MT reports, may be skewed by patriotic motivation to report knowledge of an Indian language; but the Marathi-speaking minority in Madras and the Oriya-speaking minority in Calcutta report patterns of bilingual behavior which are very similar to the English MT groups in these cities.

In Madras, Tamil has no competitor as an OT, and all linguistic minorities report a high percentage of OT use. Further, it is the only city in the sample in which the English-speaking minority reports the regional language in preference to Hindi. These facts provide no evidence for the viability of Hindi as a lingua franca in Madras; and, given the continued strong resistance to Hindi throughout the nineteen-fifties, there should be little expectation that more recent figures will show much change in the patterns of communication in the city.

In Calcutta, it is possible that the number of bilingual Bengali MT residents of the city is abnormally low. At the time the census was gathered, the city had recently absorbed large numbers of monolingual Bengali speakers from rural heartland districts in East Pakistan. Nevertheless, the bilingual behavior reported by the Oriya minority, who may be expected to have a high degree of bilingual contact (50,000 out of 52,000 are males), shows a strong preference for Bengali as a second language. In Orissa, the

Oriya speaking homeland, Oriya MT speakers show a very low degree of bilingual behavior, but are twice as likely to return Hindi as Bengali.

It would seem, therefore, that for this linguistic minority at least, Hindi does not function as a lingua franca in the city. The contrary evidence of the English MT speakers' behavior may be attributed to patriotic motives in reporting an Indian language and is probably limited mostly to the pidgin variety. Like Madras, Calcutta gives little evidence of lying within the domain of Hindi as a lingua franca.

Telugu is barely the majority language in Hyderabad. Its closest competitor as a MT is Hindi (although of course the majority of reports in Hyderabad identified the language as Urdu). The Telugus show a relatively high degree of bilingualism, with Hindi having no competitor. Hyderabad shares with Madras the characteristic that each of its major language groups reports primarily knowledge of the other's language. The bilingual behavior of the Hindi group is ascribable in part to the concentration of government officials and to the large number of Muslim shopkeepers and tradesmen.

The figures, however, probably do not accurately reflect the current patterns of linguistic behavior in the city, because they antedate the establishment of Hyderabad as the capital of the Andhra State and therefore do not include the considerable influx of government servants from the coastal districts who came to Hyderabad in the late fifties. These have had a strong impact on the city. Older generation speakers native to the city have received much of their education in Urdu medium schools and regard it as a language of cosmopolitanism and politesse. (It is not uncommon for such speakers to resort to code-switching to Urdu, either to avoid an awkward situation in Telugu, such as choosing the correct word to refer to a friend's wife, or to pay extravagant compliments.) The new arrivals, coming from government offices in Madras, or from Telugu heartland districts, where only one person in a hundred reports any bilingual behavior, have probably lowered the overall figures for Hindi use by Telugu MT speakers. They have also, incidently, set off a reaction of regional dialect loyalty among Hyderabad intellectuals, who regard coastal dialects, "pure Telugu" as its speakers view it, as symbolic of the economic threat which the new arrivals pose for the local inhabitants. In general, it would be difficult to say that Hyderabad lies now within the domain of Hindi as a lingua franca, unless, contrary to my expectations, the new census shows a rise in Telugu MT reports of Hindi as a second language.

On the basis of the census figures, only Bombay gives evidence of strength for Hindi in a cosmopolitan center outside the Hindi-speaking states. Like Calcutta and Madras, Bombay shows a relatively small proportion of reported bilingual behavior for the most widespread language. English MT speakers report a relatively low incidence of bilingual behavior, comparable to that in Calcutta, and unlike the high degree of bilingualism characteristic of English MT speakers in Madras and Hyderabad. It is possible that these figures reflect the continued extensive use of English as a lingua franca, both in Calcutta and Bombay, in the world of business and commerce.

On the other hand, there are distinct differences in the reported bilingual behavior of other minority groups. Gujarati and Hindi speakers account for approximately

the same proportion of the population of the city, but the Gujarati speaker is more than twice as likely to report use of a second language. Further, while urban speakers of both Gujarati and Marathi are much more likely to be bilingual than their rural counterparts, the overwhelming reports of both MT groups show use of Hindi as a second language. At the same time, Hindi speakers are less likely to report use of an OT than rural speakers in the state. Clearly, there is much less pressure on the Hindi speaker in the city to speak a second language. The low bilingual reports for Hindi I take to be an indication of the effectiveness of Hindi as a means of communication. It is possible that the high degree of tension between the Marathi speaker who regards the city proprietorially and the economically powerful Gujarati speaking traders fosters the use of Hindi as a neutral code which commits neither to the other's regional language.

The difference between the bilingual behavior of the Tamil and Telugu minority groups is difficult to account for. The Tamils show a higher incidence of bilingualism and a distinct preference for Hindi, which would bear out the notion that Hindi has some status as a lingua franca in the city. The Telugus, however, are as likely to know Marathi as Hindi. Possibly the Telugu evidence is misleading. The reports of Hindi as an OT may be taken as evidence of lingua franca usage and much of the Marathi bilingualism may be ascribed to the transfer to the city of borderland bilingualism from the mixed Marathi and Telugu speaking districts of Western Andhra and Eastern Maharashtra. The Telugus share a long border with Marathi, while the Tamils do not. If this notion is correct, the behavior of linguistic minority groups in the city supports the conclusion that in Bombay, which is linguistically the most diverse city in the sample, Hindi has some claim to currency as a lingua franca. In view of the position of Hindi in the city which these figures indicate, it seems likely that more recent data will show a gain in strength.

In summary, quite apart from the internal problems of establishing a standard variety of Hindi and the propaganda problems of exporting a standard Hindi to the non-Hindi speaking areas of the country, the census figures show little evidence for the assertion that Hindi is already known (or at least is already in daily use) throughout the country. Bombay alone of all the major cities which lie outside the Hindi heartland provides evidence to support the claim that Hindi functions as a lingua franca. Madras clearly and Calcutta probably lie outside its domain. Hyderabad occupies a middle ground though it is likely that in the future its profile will resemble that of Madras rather than Bombay.

#### URBAN BILINGUALISM IN INDIA (1951 CENSUS)

The first entry may be read as follows: In Greater Bombay Mother Tongue speakers of Marathi constitute 44 per cent of the population. Of these, 8 percent reported daily use of some other Indian language. Of this 8 per cent, 77 per cent reported Hindi, 14 per cent reported Gujarati. Where percentages are not given, they were either considered irrelevant or were vanishingly small.

	<i>MT</i>	<i>%Pop</i>	<i>OT%</i>	<i>OT</i>	
<i>Greater Bombay</i> (Pop 2.8 millions)	Marathi	44	8	Hindi	77
				Gujarati	14
	Gujarati	18	19	Hindi	67
				Marathi	31
	Hindi	17	9	Marathi	49
				Gujarati	34
	English	—	37	Hindi	66
				Marathi	21
				Gujarati	2
	Tamil	—	32	Hindi	66
				Marathi	9
	Telugu	—	19	Hindi	43
				Marathi	43
<i>Bombay State</i>	Marathi	—	3	Hindi	37
				Gujarati	15
	Gujarati	—	3	Hindi	52
				Marathi	39
	Hindi	—	46	Marathi	51
				Gujarati	23
<i>Calcutta</i> (Pop. 2.5 millions)	Bengali	66	4	Hindi	94
	Hindi	27	9	Bengali	95
	Oriya	—	28	Bengali	83
				Hindi	13
	English	—	33	Hindi	88
				Bengali	11
<i>West Bengal</i>	Bengali	—	2	Hindi	93
<i>Madras</i> (Pop. 1.4 millions)	Tamil	68	9	Telugu	64
				Hindi	15
	Telugu	16	56	Tamil	97
				Hindi	2
	Hindi	—	48	Tamil	88
				Telugu	7
	Marathi	—	63	Tamil	83
				Hindi	9
	English	—	61	Tamil	90
				Hindi	5
<i>Madras State</i>	Tamil	—	3	Telugu	70
				Hindi	8
<i>Hyderabad City</i> (Pop. 1.5 millions)	Telugu	53	17	Hindi	95
	Hindi	37	12	Telugu	87
	English	—	66	Hindi	71
				Telugu	16
<i>Hyderabad State and Madras State</i>	Telugu	—	11	Hindi	11
				Tamil	71

## DISCUSSION

GUMPERZ: I understand that your figures are based on the 1951 census. I have the impression that during the last decade there have been some very important changes in language distribution. The new census report should be very valuable for additional data. I am also a little surprised about your Calcutta figures. Hindi obviously is the bazaar language of Calcutta. Do you think that the census figures were skewed by the way in which questions were asked?

KELLEY: I think there is another thing involved in the Calcutta figures. In the 1951 Census, the Bengalis were already aware of what they are pleased to call Hindi imperialism, and there may have been a good deal of reluctance to report ANY second language, and specifically Hindi.

PAPER: In your work with the Census, and from your knowledge of what people in various social sciences have done with these 1951 census figures, would you rate the Census as very reliable, sort of reliable, or unreliable?

KELLEY: You can't rate the Census as a whole, you have to rate it by states. I think, as I have just said, that there may be some question as to the reliability of second-language figures in West Bengal. I know it is reliable in Madras, and I think it is reliable in Maharashtra, although there again there was pressure for reorganization at the time the census was taken, so that the question of Mahagujarat is involved in the Bombay state figures. I don't see how that would particularly affect bilingualism figures for the city itself, though it might affect mother-tongue reports. And of course, there you can draw no conclusions from the 1951 Census about the Hindi-speaking areas themselves, since that was the height of the attempts to increase the number of Hindi mother-tongue speakers as much as possible.

PAPER: At the International Congress of Orientalists in January 1964, in New Delhi, I was struck by the fact that there was never any attempt to use Hindi officially, in the organizational meetings, plenary sessions, etc. — English was the language of communication. There wasn't even a modest invocation or obeisance in the direction of Hindi. Some speakers — the President, the Secretary-General, and other officials of the Congress — quoted from Sanskrit, Persian, maybe from Arabic occasionally, but there wasn't the slightest attempt to use Hindi in any way. I thought this was a rather unusual situation, because here was an opportunity for the Ministry of Education, at the first International Congress of Orientalists that met east of Istanbul — and yet there was no attempt to nationalize.

KELLEY: Well, my favorite sign, which is in English, hangs in the post office at Poona; it reads, "Please send your telegrams in Hindi and help popularize the national language."

FERGUSON: This paper could have been presented by someone whose loyalties were in South India; so since I have some loyalties to Bengal, I would like to make a comment or two. In the first place, I agree with Dr. Gumperz' observation and yours, that Hindi was probably underplayed in these figures from Calcutta. But it is probably

not so much from fear of Hindi's being established as the national language, as it is a result of the quite traditional attitude in Calcutta, that Hindi is some sort of bazaar language. It's good for talking to clerks, or servants, or drivers, and there are a number of situations in which it is quite useful, but it certainly isn't considered a serious language that you'd be concerned with on a national scale.

My other 'pro-Bengali' comment is about the speakers of Oriya: it's my assumption that any educated speaker of Oriya naturally learns Bengali, and you've given us the statistic here that most speakers of Oriya in Orissa learn Hindi as a second language. I wondered if this had something to do with education. That is, the educated people would learn Bengali as a second language, and perhaps the uneducated people would learn some variety of Hindi.

So much for 'pro-Bengali' comments; suppose we try to make 'pro-Hindi' comments on this paper, because this is essentially a kind of 'anti-Hindi' paper.

KELLEY: A realistic paper.

FERGUSON: Well — instead of the question "Are there any major cities in the country where Hindi is actually a lingua-franca, apart from Hindi-speaking territory?" — you might ask, "If you look all over the country as a whole and measure the use of second languages everywhere, which one is more widely used as a second language than any other?" I am sure the statistics would support Hindi as meeting that criterion.

KELLEY: Of course, I'm dealing here only with cities, and there are all sorts of profiles of bilingual behavior in India. Bill McCormack and I have tried to make some statements (between ourselves at least) about prestige factors. You have learning of a prestige language, for example where Tulu speakers learn Kannada; or where a community is maintaining its identity for reasons of caste loyalty or something of the sort, which would be true of Marathi speakers in Tanjore. And both of these have kind of a prestige-feeling motive for bilingualism. But most commonly, I suppose, bilingual behavior is of the borderland type, which is prestige-neutral, but which is found everywhere along linguistic borders. And while what you say may be true, it would certainly not be the case, in the districts where Telugu borders on Marathi, that the favored mother tongue would be Hindi. And it is certainly not the case, if you are trying to equip yourself with a second language for travel in the South, that Hindi is a useful one; because as soon as you get out of the cities, you find that only the post-master knows English, and nobody else knows anything else except the regional language. It is for this reason that I concentrated on the cities, and also because I feel that it is primarily in the cities that the need for a lingua franca, in a practical sense, is most pressing.

GUMPERZ: I believe that the importance of this type of analysis lies in the information it provides about change. Somebody mentioned that the census is not the most reliable index of change. I believe that the census is important if you know how to evaluate it. There are other indices of change that might be used, however. One might inquire into the volume of book sales and the number of copies printed. I have been surprised at the small number of copies printed of what I would call 'popular' books

in Hindi. If we take a Bengali author such as Sarat Chandra Chatterji, we find that the number of copies printed of his books is almost equal to that of Prem Chand, the most popular of modern Hindi writers. In view of the fact that there are about 180 million speakers of Hindi to 80 million speakers of Bengali, these facts are somewhat surprising. Other measures that could be used would be newspaper circulation, magazine circulation, etc.

KELLEY: Well, I regard the argument from richness of literature as being a propaganda argument, but this doesn't detract from its effectiveness. I think that what you said is probably true. But to make another statement in favor of Hindi, one very successful vehicle for its spread is the movies. Lots of people in South India are becoming to a certain extent passive bilinguals because of their frequent attendance at the well-distributed Hindi movies, as against the relatively poor distribution of movies made in the regional languages. It is very difficult in India to get to see any of Satyajit Ray's Trilogy, since they were Bengali films, and, therefore, not very widely shown outside of Bengal.

HYMES: In terms of predicting the future of the situation, this resembles the Guaraní-Spanish situation, in that you seem to have, on the one hand, an ideal pattern supported by the Central Government, which recommends and insists that Hindi is the predominant language, while there is on the other hand the likelihood that for a long time the actual pattern of usage will be quite different.

KELLEY: Yes, and of course even within the government there is recognition of the need for a continued use of English, alongside the ideal of using Hindi. This duality exists within the government itself.

P. Ivić: Prof. Gumperz has mentioned one more matter that could be a very useful criterion — the number of books published, and of course of books SOLD in various areas in various languages. I would like to ask also about the public signs. We must, of course, distinguish between two categories of these signs — those put up by the authorities and those put up by private persons. Only this second kind of sign can be indicative of the usefulness of a language in a given environment.

KELLEY: Signs are in the regional language and in English, and that's probably a very good index of the retention of English as a lingua franca. This would be true even in very small towns, of both private and official signs. But of course, official signs would tend to be in English anyway, particularly outside the Hindi area.

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## NATIONAL SOCIOLINGUISTIC PROFILE FORMULAS

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Can a condensed, algebraic formula present the sociolinguistic profile of a nation adequately for comparative purposes? It is assumed here that a full-scale description of the language situation in a given country constitutes a useful and important body of data for social scientists of various interests. The question that is raised is whether it is feasible to summarize such a description in a quasi-mathematical way which will make it more convenient in characterizing a nation and more helpful for cross-national comparisons.

The term "language situation" as used here refers to the total configuration of language use at a given time and place, including such data as how many and what kinds of languages are spoken in the area by how many people, under what circumstances, and what the attitudes and beliefs about languages held by the members of the community are. A "sociolinguistic profile" is a special summary description of the language situation based in part on a series of indices and classifications.

A number of sociolinguistic indices and taxonomies have been suggested in the last few years. Greenberg (1956) proposed eight indices of "linguistic diversity" which would provide, in a simple numerical form, information about the variety of languages in use in a given area. Ferguson (1962) suggested crude scales of rating languages by degree of standardization and the extent of written use, based in large part on Kloss' views (1952, 24-31) of language development. Stewart (1962) devised a sociolinguistic "typology" of languages which classifies languages according to type and function in a given political unit. Ruston in the prepublication version of a book on developing countries (1963) presented a sociolinguistic classification of "linguistic constellations" in emerging countries. None of these indices and classifications have yet become widely used, although several of them are now being tried out in sociolinguistic research projects.<sup>1</sup>

In the present paper a kind of profile formula is offered as an answer to the question asked in the first sentence. It is based, not on a comprehensive theoretical frame of reference, but on the simple need to summarize and compare mentioned above, and

<sup>1</sup> For example, Greenberg's "index H" is being used by Stanley Lieberman (1964, 1965) in his sociolinguistic analysis of Canadian bilingualism, and Stewart's typology has been used in studies of language situations in the Caribbean area.

was developed in class discussions which concerned the preparation of students' papers describing language situations in various countries.<sup>2</sup>

First of all, it seemed clear that in a summary presentation some languages should simply be disregarded as not of sufficient significance in the total sociolinguistic picture, important and interesting though they might be from some other points of view. Of the languages to be included in the statements, some seemed to be clearly of major importance in the process of national communication, others of less importance, and still others of little direct communicative importance, but with special statuses which gave them sufficient importance to be included. These three kinds of languages may conveniently and transparently be called major language (Lmaj), minor languages (Lmin), and languages of special status (Lspec).

While it was generally clear which languages belonged in which category, in any given country for which we had a substantial amount of information, it was not easy to formalize the criteria which had determined the assignment. Discussion and experimentation led eventually to the following definitions:

A MAJOR LANGUAGE of a given country is a language which has one or more of the following characteristics:

- (a) It is spoken as a native language by more than 25% of the population or by more than 1,000,000 people. Example: Quechua in Bolivia, where roughly a third of the population speaks Quechua, but Spanish is the only official language and the language of education.
- (b) It is an official language of the nation. Example: Irish in Eire, where only 3% of the population speak Irish natively, but it is an official language.
- (c) It is the language of education of over 50% of the secondary school graduates of the nation. Example: English in Ethiopia, where only a negligible percentage of the population speaks English natively and Amharic is the official language, but English is the medium of instruction in most of the secondary schools and higher education in the country.

A MINOR LANGUAGE of a given country is a language which has one or more of the following characteristics:

- (a) It is spoken as a native language by no more than 25% of the population and by either more than 5% or more than 100,000 people. Example: Basque in Spain.
- (b) It is used as a medium of instruction above the first years of primary school, having textbooks other than primers published in it. Example: Dagbane in Ghana.

A LANGUAGE OF SPECIAL STATUS in a given country is one which does not fall in the two preceding categories, but is used in one of the following ways:

<sup>2</sup> The students' national profile studies were part of the work of courses in Sociolinguistics offered at the University of Washington (Summers 1962, 1963) and Georgetown University (1963-64).

- (a) It is widely used for religious purposes. Example: Pali in Ceylon, where it is the language of the Buddhist scriptures and is widely studied by monks and scholars.
- (b) It is widely used for literary purposes. Example: "Classical" Chinese in Taiwan, used for some forms of modern literature and studied for the classics of older Chinese literature.
- (c) It is widely taught as a subject in secondary schools. Example: French in Spain, where most secondary school students study French as a foreign language.
- (d) It is used by a substantial number of people as a lingua franca within the country. Example: Pidgin English in Liberia, used for inter-tribal communication along main transportation routes.
- (e) It functions as a major language for an age-sector of the population. Example: Japanese in Taiwan, where most educated people in the age-group 35-55 were educated in Japanese and still use it for a variety of purposes.

A simple formula based on this threefold classification of languages within a nation offers basic information on the number of languages used in a nation. For example, in Spain there are two major languages: Spanish, which is the official language and the language of education and wider communication throughout the country; and Catalan, which is the native language of 5,000,000 people in the northeastern part of the country and has been used fairly extensively for literary purposes. There is one minor language,<sup>3</sup> Basque, which is the native language of 800,000 in the north and northwest part of the country. There are two languages of special status: Latin, which is widely used for religious purposes in the Catholic Church, and French, which is studied by most students in secondary schools. Some of these facts are summarized in the simple formula:

$$(1) \quad 5L = 2L_{maj} + 1L_{min} + 2L_{spec}$$

A formula of this kind, however, hardly offers enough information to be of real value for comparative purposes, since so little information is given about the type and function of the respective languages. This additional information can be specified simply and directly by using Stewart's typology. Stewart classified languages into seven types — two of which, his artificial and marginal languages, can be disregarded here. The five basic types are:

- Vernacular (V): The unstandardized native language of a speech community.
- Standard (S): A Vernacular which has been standardized.
- Classical (C): A Standard which has died out as a native language.
- Pidgin (P): A hybrid language which combines the lexical stock of one language with the grammatical structure of another language or group of languages.

<sup>3</sup> Possibly Galician should be added as a second minor language, since it is generally classified linguistically as a dialect of Portuguese which has been under strong Spanish influence for a long time.

Creole (K): A Pidgin which has become the native language of a speech community.

It is generally a simple matter to classify a particular language as one of these types, although the line between V and S is sometimes difficult to draw. For our purposes a language will be regarded as S rather than V only if it has reached the "normal" levels of standardization and use for written purposes labelled St 1 and W 1 by Ferguson (1962). The formula for Spain, incorporating the notation of language types, now reads:

$$(2) \quad 5L = 2L_{maj}(2S) + 1L_{min}(V) + 2L_{spec}(C, S).$$

The added information about the "types" is less important, however, than information about the functions of the respective languages in the life of the nation. Of the seven functions suggested by Stewart and represented by lower-case letters, five have proved with slight modification to be useful in the kind of sociolinguistic analysis attempted here, and two additional functions have been added. The revised list is as follows:

- g: Used primarily for communication within a particular speech community, marking it as an identifiable group in the nation.
- o: Used for official purposes: either designated by law as official or used for general governmental, educational, and military purposes at the national level.
- w: Used as a lingua franca or language of wider communication within the nation (Stewart did not distinguish between national lingua francas and languages used for international communication; see *i* below.)
- e: Used for educational purposes above the first years of primary school, having subject matter textbooks published in it. (More careful specification of *e* than Stewart indicated proved desirable. Cf. the definition of "minor" languages above.)
- r: Widely used for religious purposes.
- i: Used "internationally" as a language of wider communication with other nations.
- s: Widely studied as a subject in schools.

The expanded formula for Spain now reads:

$$(3) \quad 5L = 2L_{maj}(So, Sg) + 1L_{min}(Vg) + 2L_{spec}(Cr, Ss)$$

The Stewart typology makes provision for one additional bit of information. If a language exists in two varieties in the kind of functional complementation called diglossia (Ferguson 1959), this is shown by a colon with C or S on the left and V or K on the right. Thus, for example, in Morocco Arabic is represented by C:Vorw, where the lower case "function" letters refer in part to the C variety and in part to the V variety, in accordance with the characteristics of diglossia.

Probably one of the most important indicators of language problems in a nation is the number of major languages it has: the more there are, the more difficult national

communication would seem to be. Probably an even more important indicator, however, is the presence or absence of a nationally dominant language. In the sample formulas here the symbol for a major language will be in boldface if the language represented is spoken as first or second language by over three-quarters of the population. Of the fourteen sample national formulas in this paper, four show a dominant language in this sense: Finland, Spain, Morocco, and Mexico; Taiwan belongs in this category if Mandarin Chinese is spoken as a second language by enough Amoy Chinese speakers there.

In developing profile formulas for certain countries, the problem arose of how to represent the fact that the total number of speakers of a group of languages might be quite high even though the speakers of any one of them might be well below the minimum for minor languages. In Ethiopia, for example, such languages as Bilen, Awiya, Afar-Saho, and Harari have numbers of speakers ranging from a few thousand to as many as 50,000, but the total of all may be nearly a million and constitutes a bloc which should be included in the formula. Such substantial blocs of small language communities are regarded as a single language element for the purpose of the profile formulas; they are designated by a raised plus after a number and, in the parenthetical enumerations, by a capital letter enclosed in braces, e.g. {V}. The letter is usually V, but may be S or unspecified L when appropriate. Thus the formula for Ethiopia includes the component  $5^+ \text{Lmin(Sei, 4Vg, \{V\})}$ . If the total number of speakers in the bloc should reach the minimum figure for a major language, this element would be included in that part of the formula; such would possibly be the case in a country like the Cameroun Republic.

The formulas as presented here fail to show several kinds of information important in the analysis of a language situation, but it proved difficult to devise a symbolization which would not make the formulas too complicated to be readily usable. One kind of information omitted is the extent of dialect diversity within a language, another is the amount of bilingualism or multilingualism among the inhabitants of the country. Also omitted is the difference, which seems of importance in a number of countries, between indigenous and immigrant languages. Finally, no information is offered on the nature of the writing systems used, or on the degree of literacy of the population.

The fourteen countries whose formulas are listed below were chosen chiefly on the basis of availability of data, but some effort was made to have representative coverage of areas and kinds of sociolinguistic profiles. None of the sociolinguistic "giants" (China, India, Indonesia, USSR) were included, but in principle it would be possible to devise formulas for them in spite of their complexity.

	Total	Lmaj	Lmin	Lspec
EUROPE				
Belgium	5	= 2(Sowi, So)	+ 1(Sgs)	+ 2(Crs, Ss)
Finland	4	= 2(So, Soi)	+ 0	+ 2(2Ss)

Hungary	5 = 1(So)	+ 2(Sg, Sgsi)	+ 2(Crs, Ssi)
Spain	5 = 2(So, Sg)	+ 1(Vg)	+ 2(Crs, Ss)
AFRICA			
Ethiopia	10 <sup>+</sup> = 4(Sow, Sg, Vg, Sei)	+ 4 <sup>+</sup> (4Vg, {V})	+ 2(2Cr)
Ghana	9 <sup>+</sup> = 2(Sowi, Sge)	+ 6 <sup>+</sup> (2Sge, 3Vg, Ve, {V})	+ 1(Crs)
Morocco	5 = 3(Corw:V, Sei, Vg)	+ 2(2Vg)	+ 0
ASIA			
Ceylon	5 = 3(So, Sg, S(o)ei)	+ 0	+ 2(2Cr)
Japan	3 = 1(So)	+ 1(Sg)	+ 1(Ssi)
Philippines	11 <sup>+</sup> = 9(Sowi, Sog, Sg, 6Vg)	+ 0 <sup>+</sup> ({V})	+ 2(Cr, S)
Taiwan	5 <sup>+</sup> = 3(Sow, 2Vg)	+ 0 <sup>+</sup> ({V})	+ 1(C1, Ssi)
AMERICA			
Bolivia	4 <sup>+</sup> = 3(So, 2Vg)	+ 0 <sup>+</sup> ({V})	+ 1(Cr)
Mexico	10 <sup>+</sup> = 1(So)	+ 6 <sup>+</sup> (6Vg, {V})	+ 3(Cr, 2Ss)
Paraguay	3 = 2(So, Vg)	+ 0	+ 1(Cr)

In an appendix, sample summary descriptions of the language situations in each of four countries are provided in order to explain and justify the formulas. In some respects the summaries themselves are more directly informative and even perhaps more usefully comparable from country to country, which would suggest the need to review or replace the kind of formula used here. It seems profitable, however, to use this kind of formula as a starting point in giving a fully satisfactory answer to the question posed at the beginning of the paper.

#### APPENDIX

**BELGIUM**, a country of Western Europe situated on the Atlantic coast and bounded by Netherlands, Germany, Luxembourg, and France, has an area of about 12,000 square miles and a population of about nine million. There are two major languages: Flemish, spoken natively by about 53% of the population, and French, 42%. About a third of the Flemish speakers are bilingual in French, and French has a dominant though declining position in national life. German is the native language of about 2% of the population and has a legal status in certain parts of the country. Flemish and French are both official languages: government administration, military communication, and laws are bilingual. There is a "linguistic frontier" north of which the official language of local administration is Flemish, and south of which it is French; the capital, Brussels, is officially bilingual although largely French-speaking. The medium of instruction in schools is the locally dominant language, with provision for use of the other language depending on the number of students requesting it (25 students per grade for Flemish, 6 for French). The other language is taught as a subject, generally beginning at the third year. A foreign language (English or German) is required for six years in the secondary schools. One university is Flemish, one French, and two bilingual. Latin is used for religious purposes in the Roman Catholic Church, and is required for six years in the Classical division in secondary schools (about 60%).

**FINLAND** is the easternmost of the Scandinavian countries, bounded by Sweden and Norway on the west and the Soviet Union on the east; its area is about 130,000 square miles, and the population is about 4.5 million. About 90% of the inhabitants are native speakers of Finnish,

the remainder of Swedish; both languages are official for use in Parliament, laws, and higher education. The medium of instruction for lower education is determined locally by percentage of population: schools in the minority language must be provided if the number of speakers in the community is over 12%, but are eliminated if the number of speakers falls below 8%. Where one of these languages is the medium, the other is taught as a subject in secondary schools, i.e. from the 5th to the 12th year. Beginning with the sixth year, a foreign language, usually English (60%?) or German (40%?), is studied, and in the last three years a second foreign language is added. Swedish is used for international communication with the other Scandinavian countries.

ETHIOPIA, located in the eastern "horn" of Africa, covers an area of about 450,000 square miles (including the federated territory of Eritrea in the north), with total population perhaps about 12,000,000.<sup>4</sup> The major languages spoken natively are Amharic, the official language (3 to 5 million), Tigrinya (1.3 million), Galla (2.5 million). Of these, Amharic is used extensively for written purposes, Tigrinya somewhat less so, and Galla is not normally used for writing. English is steadily increasing in importance, with most secondary and higher education now using it as the medium. Out of nearly a score of less important languages, four are spoken by a quarter of a million or more: Tigre, Gurage, Somali, and Sidama; of them Somali is the chief language of neighboring Somalia, Tigre is also spoken in parts of the Sudan, and the other two are confined to Ethiopia; Sidama is regarded by some as a group of closely related languages. There are other languages spoken by smaller numbers of people, totalling altogether possibly a million speakers. The language of the Ethiopian church is a Classical language, Ge'ez, and (Classical) Arabic is the religious language of the Muslims. Amharic, Tigrinya, and Ge'ez are all written in the Ethiopic alphabet, which consists of about 30 consonant letters which occur with different vowel diacritics and are written from left to right; there is a special word-divider symbol.

MEXICO, one of the largest Latin American countries, is bounded on the north by the U.S.A. and on the south by Guatemala; its area is about 760,000 square miles, and the population is about 35,000,000. About 90% of the population are native speakers of Spanish; the remaining 3,500,000 are native speakers of Indian languages. Of about fifty Indian languages in the country, six (Nahuatl, Yucatec, Otomí, Zapotec, Mixtec, Totonac) have over 100,000 speakers each, and together account for about half of the speakers of Indian languages. Many, perhaps half, of the Indian speakers are bilingual — sometimes in two Indian languages, more often in their native language and Spanish. A number of the Indian languages are used as the medium of education in the first years of school and have primers published in them; several are used somewhat more extensively for written purposes. The study of a foreign language is required in the secondary schools and the most widely studied are English (80%?) and French (20%?). Private schools exist with French or English as the medium of instruction. Latin is used for religious purposes in the Roman Catholic Church.

#### DISCUSSION

FERGUSON: What I want to present is an unsatisfactory solution to a problem that some of my students and I have been concerned with for several years: how to compare nations in some useful way in sociolinguistic matters. This grew out of a concern with educational and other problems related to language in developing countries. We wanted to know how to describe a nation in a way that would be useful for comparative purposes within linguistics or sociolinguistics, and also for correlation with economics,

<sup>4</sup> Demographic estimates for Ethiopia vary widely; the figures here are based on Ullendorff 1960.

political science, and so on. After all, this is a very small universe of discourse — there are somewhere between 100 and 200 countries in the world — and we have other people's classifications of various kinds of political development, and so on. The procedure was to assign students term papers on the 'sociolinguistic situation' or 'language situation' of a particular country. The student was obliged to find out as much as he could about the sociolinguistic situation of that country from whatever sources of information were available, and then phrase his description in any way that he found useful. We subjected the papers to class discussion, and gradually, working through three courses in this way, we began to evolve a kind of format for a summary of language situations: a one-page summary in ordinary English like the four sample descriptions in this paper. But it is hard to compare these one-page summaries, no matter how clear and useful they may be, and so we were tempted to try to put this down in some kind of abbreviated notation. What I'm presenting here today is the kind of notation which has come out of these term papers and class discussions. Perhaps it is useful as a starting point towards giving a more satisfactory answer to this question of how to summarize the language situation in a country for comparative purposes within linguistics or sociolinguistics, and for correlating with other descriptions by economists or political scientists.

HYMES: Maybe in Mexico you should indicate when a distinctive region is associated with a language. There's a lot of difference between a hundred thousand speakers scattered around, as opposed to a hundred thousand in a block.

FERGUSON: That's true; if they're clustered in one place, obviously this in some sense makes them more important than if they are minority groups in a number of places.

DILLON: As a reference was made to Ireland, I feel challenged. I'm relieved to hear that notice is being taken of our situation, and I'm sorry to say that an estimate of 3% of Irish speakers in my opinion would be pretty high — it's nearer 1%. But the fact is that both Irish and English are recognized as official languages.

The Celtic group of languages raises all sorts of problems. In Ireland 1% of the people speak the language from birth. We have the fact that the language is one of the official languages of the country. But the curious thing is that, in the native-speaking areas, the language has no prestige; whereas, when you get to Dublin, the attitude is that it's our national language and our great pride. The contrast with Scotland is interesting. In Scotland, Gaelic is not the official language. But it isn't only the peasants who speak Gaelic — the lawyer speaks it, the teacher speaks it, the minister of the church speaks it — Gaelic is associated in peoples' minds with those in high position. They hate Lowlanders, and they are proud of being Highlanders, and there are even some of them proud of the fact that they're descended from ancient Irish kings. There is quite a different attitude from that in Ireland. And of course the number of speakers is much higher — there are more than twice as many speakers of the Scottish dialect in Scotland as there are of Irish in Ireland. There are probably more speakers of Irish in Boston than there are in Dublin.



What are you going to do with a language like Welsh? Welsh is spoken by nearly a million people still. It's used in the courts now; it's used in the schools; it's used in the University. It's not used as an official language. Is it a major or a minor language? I don't know where you'd put it in. What about Manx, which is extinct, but is still recognized as an official language on public occasions? There is then the question of Breton. Breton is also widely spoken by nearly a million people. It is permitted to be taught in the primary schools, although the French hate it and do all they can to suppress it. But, I think, in the Catholic private schools it may be favored, and of course it's a literary language now. These are marginal cases, and I'm wondering whether you considered them or what you do with them.

FERGUSON: Both Welsh and Breton we regarded as minor languages, looking at the U.K. as a nation and at France as a nation. But they are certainly on the borderline between being minor and major languages.

DILLON: When I was last in Wales, there was a campaign going on — a sort of strike — by which students were parking their cars in no-parking places in order to get summonsed by the police — and to insist on their right of being summonsed in Welsh.

BRIGHT: When you were talking about Stewart's classification of types of languages, you mentioned some difference of opinion on the definition of pidgin, and you gave your own definition of a pidgin language as a hybrid. Now I notice that Professor Kelley spoke of Bazaar Hindustani in his paper as pidgin, and I assume that that is not a hybrid, but a grammatically simplified form of Hindi. How then would you classify Bazaar Hindustani, so far as you know the case, and what is the difference between you and Stewart in your definitions of pidgin?

FERGUSON: On this point, Stewart is being careful; he didn't want to commit himself as to whether a pidgin had to be a hybrid or not. I don't know what his decision would be on Bazaar Hindustani, but by his definition as I understand it, he would probably allow it.

KELLEY: I would rather not commit myself on pidgin the way you do — that it should have one source lexically and another grammatically. I would prefer to leave the way open to hybridization, both grammatical and lexical. In the case of Bazaar Hindustani, I would expect certain lexical hybridization, because dialects of Bazaar Hindustani vary from place to place, and the source of loans is from the local dialect, very much as in Samarin's Sango.

PAPER: This paper illustrates the many kinds of information that we have never been able to get, or haven't even thought of getting. I wanted to bring up some other examples, because when I read the Mexico profile I thought something was omitted and I couldn't think of it until you were talking this afternoon. Then I remembered that Norman McQuown did his Spoken Turkish, during the Second World War, with a native speaker of Turkish in Mexico City. There is, furthermore, a large Yiddish speaking population in Mexico City. There is a Yiddish-language school system that is both the only school system that many children go to as well as a supplementary

system; there is at least one newspaper, book publication, and so forth. I suppose a similar situation exists with Lebanese Arabic in Brazil.

FERGUSON: Or with Gujarati in Kenya, or something like that. I meant to mention that we don't distinguish in these formulas between so-called 'indigenous' languages and 'immigrant' languages, which is a distinction often made very strongly in the countries we are talking about. But if, let us say, Yiddish in Mexico meets the other criteria, then it should have been put in. We knew that there were schools in Yiddish, and also schools conducted completely in French. Probably we didn't include them because the numbers were too small.

LAW: There's one thing which was left out of your formulas which is rather important to me — that is, the total number of languages reported for a particular area. We realize of course that such figures are inaccurate for many countries, because we are discovering new languages, and things of this sort. But I thought it might be worthwhile to try including it, and I would suggest you might consider including it as a denominator beneath your first number. That is, for Mexico it would be 10 over such and such a number; you are considering 10 languages out of the total number reported for the area.

FERGUSON: We did consider putting the total number of languages in somewhere, and the problem was really with the immigrant languages. In a country like Canada, for example, there are large numbers of immigrant languages. There are probably more speakers of Italian in Toronto than any city outside Rome. But the usual figures you get for the number of languages in a country don't include those languages. And I might say, while I'm mentioning immigrant languages, that this problem is not limited to the kind of immigrant groups that we have in the United States. Actually the best example we had of this was in Sarawak and North Borneo, where the percentage of Chinese is increasing rapidly; in fact, it has already reached about 30% of the population, and their birth rate is higher. So it seems that within a few decades the Chinese will far outnumber the Sea Dyaks, Land Dyaks, etc.; and yet Chinese is regarded as a foreign language, an immigrant language, both by the Chinese and the local people, even though the Chinese speakers are citizens.

HAUGEN: You might need to make provision for restoring other languages in your formulas, to give some of this other information that has been mentioned by so many speakers, and that we'd like to have in a complete survey of languages of the world. For instance, in Scandinavia, a minor language is Lappish, and Lappish doesn't get in by any of your formulas.

FERGUSON: It doesn't get in for Finland, here, for example.

HAUGEN: Nor for Sweden, or Russia, or even for Norway, although in Norway there are 30,000 speakers and it is taught in the schools. Both in Norway and Sweden there are professorships of the language in the universities. Here is a language that gets split between several countries and as a result falls out, so to speak, between the partitions of your grid. Faroese is another one, which is now a standard language and has been for a hundred years, but has a very low official status and only 30,000 speakers.

Yet it has a literature, a good dictionary, and a professorship in Copenhagen. What do we do with it?

FERGUSON: If it has a substantial literature, then it could be regarded as a special language with a special status and would be called "S1" in this case.

HAUGEN: But it doesn't have a special status. It's rather a small areal language, like some of the ones that have been already mentioned. I think the concept of area is a very important one, for a population which has a sense of cohesion and national significance, and more and more people are going to develop it, I'm sure, in the next hundred years.

FERGUSON: Of course, if Faroese were spoken in India, it would be disregarded — 30,000 people with a standard language off on a couple of islands somewhere...

P. Ivić: I am not so sure that the nation-wide group languages are of lesser importance than regional languages. Let's take the case of inter-war Poland. Yiddish was used by about 10% of the population — the Jews, scattered throughout the nation in practically all towns and cities. Thus Yiddish was a nation-wide reality; whereas the Byelorussian language, which was spoken by roughly the same number of people in Poland, was a regional reality.

I also question the definition of the 'official language'. In some countries, like Yugoslavia, there are official languages which are really nation-wide, then there are those of provincial importance, and finally there are languages which are official only in certain communities. For instance the Slovak language, having about 50,000 speakers in Yugoslavia, is official in the communities where it is spoken. Should we treat it equally with Serbo-Croatian?

FERGUSON: We did decide to include languages which were regionally official, so to speak; but that raises the question that some official languages are more official, let us say, more widely used than others. And I know that Stewart is wrestling with that problem, too, not only in the case of 'official' but in some of these others. He is thinking of some sub-divisions or additional functions, when languages of one function or another are nation-wide rather than local. But by these formulas we would include the letter "o" beside a language which was used as a regional official language.

P. Ivić: Another point is, what do the words "widely used for religious purposes" mean? Here also we need more precision. In Yugoslavia there is an Eastern Orthodox population embracing about one half of the inhabitants and using Old Church Slavonic in churches, while about 40% of the people are Roman Catholics using Latin. Obviously, these two languages should be considered as widely used. But what about Arabic, used by the Moslems who make up some 10% of the population? And finally there is a small Jewish population using Hebrew for religious purposes. Where is the limit?

FERGUSON: I don't know how to answer that question. We speculated about what we would include for the United States as widely used for religious purposes, and I think we decided on Latin, Greek, Old Church Slavonic, Hebrew — but I'm not quite sure what our criteria were.

P. Ivić: Another intricate question is that of languages widely taught in secondary schools. Would it be proper to regard, e.g., four foreign languages which are all taught widely in secondary schools as a characteristic of the nation? In Yugoslavia there are four languages of this kind — English, Russian, French, and German; and the percentage of students is now about the same (in fact, in most schools the students have the choice of two out of four). Should all four be in the list?

FERGUSON: I agree that we should have more precise limitation. It so happened that in the fourteen countries we studied here it was relatively easy to decide which languages were relatively more important than others. In Spain, for example, almost everybody takes French in schools although you can take English or German. One could even exclude Greek from Belgium, for example, although many students study Greek there; whereas you couldn't exclude Latin from the school system, since probably 60% of secondary school graduates in Belgium have had six years of Latin.

BRYAN: There is one aspect of the linguistic description of a country that you haven't touched on, and I'm wondering if you've neglected it as irrelevant, or as completely hopeless, that is whether the different languages are or are not related to each other. If you were describing the linguistic situation in Tanganyika, Swahili, the official language, and the other languages spoken there (some of them by large numbers of people, and some of course by only a few thousand) are mostly demonstrably related. And English is demonstrably not. Or in the Sudan, for instance, there are a very large number of languages, many of them demonstrably not related. Amharic and Galla are distantly related, but how distantly? Greenberg would relate them more closely than I would. Well, anybody who knows anything about African linguistic classification knows that there is at present a great debate on such matters between the British Africanists and the American Africanists. So some languages are not related, some demonstrably are, and others the linguists are still scrapping over. Are you going to try and cope with that situation at all?

FERGUSON: I should have mentioned that explicitly as one of the things unfortunately not covered in the formulas. Dialect diversity within a single language isn't shown. But even if we did include that in the formulas, I would prefer to put it in terms not of genetic relationship, but of some other measure of similarity, because what really matters here is the ease of learning the various languages, rather than some precise degree of relationship.

BRYAN: The fact of the matter is that the outlook of Swahili as the official language in Tanganyika is pretty good precisely because of its close relation to its neighbors.

MCDavid: I might mention one complicated system, dating from my Burmese avatar. As my informants of 1942-1945 reported the situation, before the war English had official status in Burma. Among the people of the country, Burmese was the dominant everyday language; there was, besides, an ecclesiastical language, Pali, and a learned language, Sanskrit. In addition, under the British administration, before Burma was separated from India, all arms of civil administration were a part of the Indian civil service. In consequence, down to independence the Burmese civil service,

railroads, and telegraph were largely manned by people of Indian descent, with Hindustani the operational language of these official functions; in the capital, Rangoon, there was more Hindustani spoken than Burmese. And in Burma, as elsewhere in southeast Asia, the Chinese proved themselves so enterprising that for all practical purposes Chinese was the language of finance and retail trade. In addition there were various ethnic minorities, of greater or less size, with more or less official recognition of their language. There was consequently some difficulty in calculating how extensively each of these languages was used.

But we can find an equally difficult situation closer to home, where it is exceptionally difficult to get accurate figures on the use of other languages than English in the United States. In an effort to discover what languages are used how extensively and by whom, Einar Haugen, Joshua Fishman and I have been pounding a number of tables. All we can say is that it is less difficult to get the figures for first-generation immigrant speakers than it is to learn the numbers of speakers of languages that have been long domiciled in this country, whether aboriginal or colonial or immigrant. According to the 1960 census, there are only twelve hundred native speakers of French in Louisiana, because the census office records only what it calls the "mother tongue of the foreign born." It presumes that every native-born American, wherever residing and of whatever parentage, is a native speaker of English. A further complication in this situation is that only one "mother tongue" is recorded for each speaker, and when in doubt the census-taker puts down the official language of the nation from which the immigrants came, so that Swiss, Luxemburgers, Belgians, and Ashkenazic Jews are not fairly described from a linguistic point of view. The simple-mindedness of the census deprives us of information about many of the interesting language situations with which we are concerned.

FERGUSON: It might interest you to know that as major languages for the United States we agreed on English, German, Italian, Spanish, Polish, Yiddish, in that order. That is, these all fulfilled the criterion of major language, which came as a surprise to those of us who were speculating about it.

On the question of the census, just try suggesting to the Bureau of Census that they should add just one more question to the questionnaires — say, what third language is spoken — and you'll realize that there's pressure from a hundred and one groups to get additional questions onto the census, all of which are said to be vital to understanding how America functions. The Bureau of the Census people are naturally slow to add questions.

FRIEDRICH: Dr. Dillon mentioned the Scotch Gaelic and Dr. Haugen the Lappish case. It seems to me that your criteria for ranking these languages and deciding whether to include them were heavily behavioristic. That is, you were looking for things that you could see and measure: do they use it in a church, use it in parliament, things of that sort. This has the virtues of objectivity, but also the weakness of avoiding things that you can't measure in a certain way. It also will elicit a cry of protest from people who think that some language matters a lot in their country.

Now maybe the Norwegians feel that Lappish matters a lot. I feel that way about Tarascan, which just happens to fall out of your study because it doesn't have a hundred thousand people; but this is a language of great symbolic significance within Mexico. It has a considerable status and prestige. Also in the U.S. there is great difference in the symbolic statuses of languages. Some are quite large, but the people don't care very much; others have high standing in certain ways. I think one could avoid the dangers of the subjective factor by perhaps working out a few standard questions which one would put to the people who are in a position to know, or to the population in question.

GARFINKEL: I have two comments. First, when I reflect on your reservations about your profile 'dimensions', I'm struck by the fact that the properties you use as rules to analyze an actual linguistic community depend, for their character as 'appropriate interpretations' of an actual case, upon the existence of members' own knowledge of 'systematic' properties of their common linguistic practices, and on members' uses of these properties as rule-like bases for conducting their everyday affairs. In this way MEMBERS' practical linguistic activities are measures which insure the 'appropriateness' with which your rules analyze a situation. However, no notice is taken of this characteristic of the linguistic community.

Second, in addition to your set of formulas for characterizing linguistic situations, there ought to be another section with the heading: "Practical advice to whomsoever might seek to insure the usefulness of the formulas to analyze the situations." Such a section, and the practical advice it contains, are dictated by our task. For example, consider the rule, " $5 = 2(\text{Sowi, So})$ " and the rest, for Belgium. Our task is to treat an actual linguistic situation in such ways as to make this rule a correct method for analyzing the actual situation. In order to accomplish this task and to assure its continued accomplishment, we depend upon the right to talk about and to MAKE something of linguistic situations and methods for analyzing them. Professionals — linguists, anthropologists, sociologists — claim such rights from and assign them to each other by reason of association membership. From a knowledge of the practices that make up the ways of offering and claiming professional competence, we can make out the advice that professionals use, that they don't need to be told about, when it comes to seeing how a situation may be treated to make a rule right.

One piece of advice runs: "Read the rule like this, ' $5 = 2(\text{Sowi, So})$ , *and so forth*'; i.e., to see the rule, ADD *and so forth*." A second piece of advice: "Read the rule so as to include, as part of what it is talking about, any and all considerations of *unless*, considerations which Any Member knows need not and cannot be cited BEFORE they are needed, though No Member is at a loss when the need is clear." We saw that come up today.

A third item of advice runs: "In the course of seeing that a rule applies, the use of *and so forth* and *unless* can introduce any matter whatsoever, thereby raising questions of where any of it ought appropriately to begin or end. Read the rule, therefore, with the proviso "Let it pass", or "Enough is enough as Anyone knows." For example,

here is Belgium. We have the question, "Does this rule apply?" Well, there is this, and this, and that, and so — well, "Let it pass, enough is enough, we have to get DONE."

Finally, I've been told of a practice for seeing that a rule applies, called "Factum valet". It is said to mean that an action otherwise prohibited by rule is to be treated as correct if it HAPPENS nevertheless.

I am not carping or finding fault. I'm quite serious. I propose that such practices — admittedly I've only given a rough and ready characterization of them — are part and parcel of what we are doing in "using a rule" or in recognizing that a rule applies, or when we're said to be following a rule.

FERGUSON: I wonder if, in presenting these alleged facts about various countries, I was misleading some of you into thinking that, if you asked a native of one of these countries, you'd get full agreement with the formula that I've presented here. I'm sure that for almost every one of the fourteen countries, there would be serious questions raised by people of the country. But it might be very useful to attach to these some statements — perhaps not quite along the line suggested by Dr. Garfinkel, but certainly somewhat similar — statements which would explain the attitudes of various segments of the population on these particular questions, because these are also part of the language situation. That is, whether a language is used as a means of education or not is one important question. Whether a large segment of the population believe it SHOULD be used as a medium of education or not is another important question. And this second kind of thing is not in the formulas at all.

GARFINKEL: I suggest that the very features you treat as shortcomings of your method, as troubles for which you seek remedies, are essential characteristics of linguistic situations and may be of critical interest in themselves. I'm arguing that linguistic phenomena, in that they consist of members' common ways of talking, are METHODS for producing organizations of everyday activities. As methods of organizational production, these common ways of talking are themselves also FEATURES of organized activities of everyday life. In their character as method and feature of organized everyday activities, linguistic phenomena are essentially equivocal phenomena. As matters stand right now, we are conducting studies of common linguistic practices as if we only lack the good sense or wit or training or mathematics to repair this equivocality, and that its repair rather than its study is required to bring common linguistic practices under the jurisdiction of rigorous methods of description. But I urge that this essential equivocality requires examination in its own right.

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